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(2008 American Accounting Association Competitive Manuscript Award Winner)

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Disclosure Quality, Cost of Capital, and Investor Welfare

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ABSTRACT: One might expect that disclosure quality improves investor welfare by reducing cost of capital. This study shows that the argument is valid only in limited circumstances. Based on a production economy with perfect competition among investors, the analysis demonstrates three points. First, cost of capital could increase with disclosure quality when new investment is sufficiently elastic. Second, there are plausible conditions under which disclosure quality reduces the welfare of current and/or new investors. Finally, cost of capital could move in opposition to the welfare of either current or new investors as disclosure quality changes.

Keywords: *cost of capital; disclosure; welfare; real effects.*

I. INTRODUCTION

Regulators and public firms are concerned about the welfare impact of *ex ante* disclosure policies. Because it is difficult to empirically measure investor welfare, a great deal of recent efforts have focused on the relation between disclosure quality and cost of capital, as an intermediate step to the ultimate goal of understanding the welfare impact of disclosure policies. For example, Arthur Levitt, the former chairman of the Securities and Exchange Commission, has claimed, "The truth is, high [accounting] standards lower the cost of capital. And that's a goal we share" (Levitt 1998, 82). This reasoning, as well as similar arguments pervasive in policy discussions, has been frequently cited as the motivation for studying the relation between disclosure quality and cost of capital. One

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This study is part of my dissertation at Yale University. I sincerely thank my advisers, Rick Antle, John Geanakoplos, Brian Mittendorf, and Shyam Sunder (Chair), for their guidance and encouragement. I also thank Jeremy Bertomeu, Ron Dye, Thomas Hemmer, Bjorn Jorgensen, Steven Kachelmeier, Christian Leuz, Dong Lou, Richard Sansing, Haresh Sapra, Phillip Stocken, Ro Verrecchia, Winnie Wen, and two anonymous referees for many helpful comments and discussions. The work has been substantially improved by the constructive suggestions I have received from the workshops at Carnegie Mellon University, Columbia University, Baruch College–CUNY, Dartmouth College, Duke University, New York University, Northwestern University, The Pennsylvania State University, The University of Chicago, University of Houston, University of Minnesota, University of Toronto, University of Pennsylvania, Yale University, and the AAA 2008 Financial Accounting and Reporting Section meeting. In addition, I am grateful for the generous financial support of the Deloitte Foundation and The University of Chicago Booth School of Business.

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interpretation of this reasoning is that cost of capital summarizes the impact of disclosure quality on investor welfare. I explicitly examine this interpretation.

Moreover, even on the relation between disclosure quality and cost of capital, there has been a gap between the empirical evidence and theoretical research. While empirical findings on the relation have been mixed, as surveyed by Healy and Palepu (2001) and Leuz and Wysocki (2007), most theoretical studies have examined a competitive, pure exchange economy and predicted that disclosure quality monotonically reduces cost of capital. Although empirical challenges may have contributed to the inconsistent empirical findings, such as the self-selection problem and measurement errors in proxies for cost of capital and disclosure quality, my model provides a theoretical explanation by introducing the investment effect of disclosure.

In sum, I address two questions. First, how does disclosure quality affect cost of capital, current investors' welfare, and new investors' welfare when disclosure influences a firm's investment decisions? Second, how well does cost of capital represent the welfare of current and/or new investors as disclosure quality changes? I first construct an economy in which disclosure affects a firm's investment decisions by influencing investors' valuations. I then identify necessary and sufficient conditions under which disclosure quality reduces cost of capital and improves the welfare of current and new investors. Finally, I compare these conditions to show that they are neither equivalent nor do they subsume each other. Therefore, cost of capital is not always synchronous with the welfare of either current or new investors in the analysis of the economic consequences of disclosure quality.

The results serve as a caveat for the usefulness of cost of capital, defined as risk premium per dollar investment, in representing investor welfare in complex settings. When a single risk-averse owner cares only about the mean and variance of a project's final cash flow and holds the investment until it pays off, she prefers the project with lower cost of capital, i.e., lower variance provided that the projects have the same means (e.g., Hadar and Russell 1969). In such a setting, one might reasonably expect that cost of capital moves in parallel with the owner's welfare. But as one moves toward a more complicated environment in which the investment is influenced by disclosure and current investors divest their holdings subsequent to disclosure, the effectiveness of cost of capital as a measure of welfare diminishes and cost of capital does not always remain consistent with investor welfare.

One complexity of the environment is the investment effect of disclosure. Disclosure affects a firm's investment by revealing its information to the market. Such disclosure influences investors' beliefs and valuations, which in turn guide the firm's investment decisions. The firm's investment decisions affect the stock price, and the stock price feeds back into the firm's investment choices, both being determined consistently in a rational expectations equilibrium.

The investment effect of disclosure is instrumental in its impact on cost of capital. Disclosure reduces investors' uncertainty about the firm's marginal profitability, such that investors would like to pay a higher price for the firm's shares *on average*. In a pure exchange economy, higher price implies lower cost of capital, and thus disclosure quality monotonically reduces cost of capital. However, when new investment is possible, as in my model, resolution of uncertainty of the firm's marginal profitability also guides the firm to adjust its investment level. As a result, both the total investment and the marginal profitability of per unit investment are affected by disclosure quality. *A priori*, it is not clear whether the cost of capital, a measure of per dollar risk premium, is higher or lower in equilibrium. The first result of my analysis shows that cost of capital increases with disclosure quality if and only if the adjustment cost of new investment is sufficiently low and the prior expected profitability of existing investment is sufficiently high.

The investment effect is also important for the welfare consequences of disclosure quality. In a pure exchange economy, disclosure only allocates the risk between current and new investors by substituting the price risk for the cash flow risk. For current investors, disclosure creates a trade-off between a higher average level and a higher volatility of the stock price. When current investors are sufficiently risk-averse relative to new investors, high disclosure quality makes current investors worse off by preventing them from transferring more risk to new investors. In a production economy, however, disclosure also facilitates the firm's investment decisions and thus improves current investors' welfare. The second result of the model demonstrates that current investors are worse off with higher disclosure quality if and only if current investors are sufficiently risk-averse relative to new investors and the adjustment cost of new investment is sufficiently high.

Finally, disclosure quality makes new investors better off only in a production economy. New investors gain surplus from trading by contributing their risk tolerance to the market. Early resolution of uncertainty reduces the amount of risk left in the market and thus decreases the demand for risk-taking capacity. As a result, new investors gain less surplus from bearing risk for current investors. In the presence of the investment effect, the overall risk of the firm's cash flow is a function of both the risk of per-unit investment and the total investment. While disclosure reduces the risk of per-unit investment, it could increase the total investment. The third result of the analysis shows that disclosure reduces new investors' welfare if and only if both the adjustment cost of new investment and the level of existing investment are sufficiently high.

The above analysis reveals that the economic forces behind the impacts of disclosure quality on cost of capital, current investors' welfare, and new investors' welfare are different and do not subsume each other. Therefore, cost of capital does not measure well the welfare of either current or new investors. In particular, disclosure quality could increase cost of capital when it increases the overall risk of the firm's cash flow. Such an endogenous increase in risk could benefit current investors if it is accompanied by a simultaneous increase in the level of the firm's cash flow, and could benefit new investors because it makes their risk tolerance more valuable.

The main contribution of the study is twofold. First, it links cost of capital to investor welfare and shows that cost of capital does not always move parallel with investor welfare. Given the prominent empirical focus on the relation between disclosure quality and cost of capital, these results help to clarify the connection between this literature and the broad policy debate on the reform of financial reporting and disclosure regulation. The other contribution is to identify the relation between disclosure quality and cost of capital in the presence of the investment effect of disclosure. This result may help sort out the mixed empirical findings on the relation between disclosure quality and cost of capital.

My analysis is couched in a setting of perfect competition among investors to highlight the role of the investment effect of disclosure. Every atomistic investor conjectures that his or her demand does not affect the price, and in equilibrium investor beliefs are fulfilled. As a result, (il)liquidity is never an issue. Alternatively, in models of *im*perfect competition, such as Diamond and Verrecchia (1991) and Lambert and Verrecchia (2009), disclosure serves to reduce cost of capital and potentially enhance welfare by improving liquidity and ameliorating adverse selection. This is a promising direction to further connect cost of capital to investor welfare.

Section II develops the model and studies the effect of disclosure quality on the distribution of the firm's cash flow. Section III examines and compares the effects of disclosure quality on cost of capital, current investors' welfare, and new investors' welfare. Section IV discusses the related literatures and some assumptions. Section V explores empirical implications of the results. Section VI concludes. All proofs are in the Appendix.

II. THE MODEL AND EQUILIBRIUM

This section describes and solves the model. It is a disclosing-and-then-trading model that allows disclosure to influence the firm's investment. After solving for the unique equilibrium, I discuss three properties of the equilibrium and in particular examine how disclosure quality changes the characteristics of the distribution of the firm's cash flow.

The Model

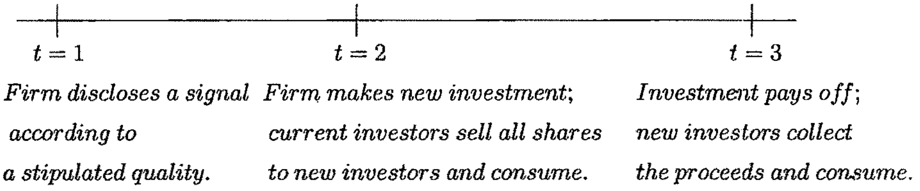
I study a large economy to allow for risk sharing in a competitive market. The number of risky assets (firms) *per capita* is finite, although the number of both investors and risky assets could be infinite. Thus, I could describe the model in terms of per capita without loss of generality. The risky shares of a representative firm are traded between current and new investors after disclosure, and the number of shares per capita is normalized to be one.[1] There is also a risk-free asset, which acts as a numeraire with a normalized return of one. Figure 1 describes the time line of events.

At $t = 1$, the firm, which has m units of existing investment, discloses a public signal about its marginal profitability, according to a pre-specified disclosure policy. Investors' prior belief about the marginal profitability is characterized by a mean μ_0 plus a future innovation $\tilde{\mu}$. Before disclosure, they perceive that $\tilde{\mu}$ has a prior distribution of $N\left(0, \frac{1}{\alpha}\right)$. The disclosure, denoted by $\tilde{y}$, provides all investors with an unbiased estimator of $\tilde{\mu}$, and takes the following form:

$$\tilde{y} = \tilde{\mu} + \tilde{\varepsilon}, \tilde{\varepsilon} \sim N\left(0, \frac{1}{\beta}\right),$$

where $\tilde{\varepsilon}$ is independent of $\tilde{\mu}$. β is the disclosure quality and the main variable of interest. As β increases, the disclosure conveys better information to investors about the firm's

FIGURE 1
The Time Line of Events



Firm discloses a signal according to a stipulated quality.

Firm makes new investment; current investors sell all shares to new investors and consume.

Investment pays off; new investors collect the proceeds and consume.

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[1] Although I only study a single-firm economy, the analysis is robust to diversification in a multi-firm economy. Based on Lambert et al. (2007), it will become clear later that I can interpret the variance of the firm's cash flow here as the covariance of the firm's cash flow with the cash flow of the market portfolio. The Bayes update rule for covariance is similar to that for variance, and thus the major proofs can go through with covariance.

marginal profitability. The model focuses on the consequences of different levels of β for cost of capital and investor welfare.

At $t = 2$, the firm makes an additional investment k to maximize its expected stock price, and current investors then sell their shares to new investors. The net cash flow from k units of new investment takes a quadratic form. Thus, new investors perceive that the firm's cash flow is as follows:

$$\tilde{F} = m(\mu_0 + \tilde{\mu}) + k\tilde{\mu} - \frac{z}{2}k^2. \tag{1}$$

For new investors, $\tilde{F}$ is the stochastic net cash flow at $t = 3$, if the firm has m units of existing investment and makes k units of new investment at $t = 2$. The first component $m(\mu_0 + \tilde{\mu})$ is the cash flow from the existing investment. The other component, $k\tilde{\mu} - \frac{z}{2}k^2$, is the *net* cash flow from the new investment k. z is the adjustment cost of new investment, capturing the degree to which disclosure quality influences the firm's investment.[2] Thus, m, μ_0, and z are fixed parameters, k is the firm's choice variable, and $\tilde{\mu}$ is the only source of uncertainty in the firm's cash flow.

After the firm makes the new investment, current investors sell all of their shares to new investors in a competitive market, consume the proceeds, and leave the market. Based on the firm's disclosure and new investment, new investors submit their demands for the firm's shares. Market clearing yields the stock price, which is the market valuation of the firm's stochastic cash flow $\tilde{F}$.[3]

At $t = 3$, the firm's investment pays off, the firm is liquidated, and new investors consume.

Both current and new investors have CARA utility functions; that is, $U(W_i) = -exp\left(-\frac{W_i}{\tau_i}\right)$, $i \in \{c, n\}$. τ_c and τ_n are the coefficients of risk tolerance of current and new investors, respectively. The subscripts c and n represent current investors and new investors, respectively.

In sum, the two decisions to be made are how much new investment to make by the firm (or equivalently by current investors) and how many shares new investors want to bid. All parties observe all parameters, including existing investment level m, new investment level k, and the public signal y. For expositional ease, I assume that all parameters are well defined except in three special economies defined later.

Equilibrium Trading Price and Optimal Investment

A rational expectations equilibrium is a pair of a trading price function $p(y)$ and an investment function $k(y)$, such that, for any signal y, the pair $(k(y), p(y))$ satisfies:

1. given $k(y)$, $p(y)$ clears the market;
2. given $p(y)$, $k(y)$ maximizes $p(y)$.

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[2] z could also be interpreted as a measure of the general economic outlook. By rescaling and rewriting the net cash flow from new investment as $k\frac{2}{z}\tilde{\mu} - k^2$, $\frac{1}{z}$ becomes a measure of the profitability of new investment.

[3] Although I describe the investment and trading as two sequential steps, the order does not matter because rational expectations guarantee that the firm's investment decisions and new investors' valuation decisions are consistent in equilibrium.

Lemma 1: For any signal y, the unique equilibrium ($k(y)$, $p(y)$) is as follows:

$$k(y) = \frac{E[\tilde{\mu}|y]}{z + \frac{2}{\tau_n} Var[\tilde{\mu}|y]} - \frac{\frac{2}{\tau_n} Var[\tilde{\mu}|y]}{z + \frac{2}{\tau_n} Var[\tilde{\mu}|y]} m,$$

$$p(y) = E[\tilde{F}|(y, k(y))] - \frac{1}{\tau_n} Var[\tilde{F}|(y, k(y))].$$

I examine three properties of this equilibrium: the investment effect, the risk allocation effect, and the overall effects of disclosure quality on the distribution of the firm's cash flow. First, the adjustment cost of new investment z measures the intensity of the investment effect. One proxy for the impact of disclosure on the firm's investment decisions is the unconditional variance of the firm's new investment ($Var[k(y)]$).

$$Var[k(y)] = \frac{Var[E[\tilde{\mu}|y]]}{\left(z + \frac{2}{\tau_n} Var[\tilde{\mu}|y]\right)^2} = \frac{Var[\tilde{\mu}] - Var[\tilde{\mu}|y]}{\left(z + \frac{2}{\tau_n} Var[\tilde{\mu}|y]\right)^2}.$$

As disclosure quality β increases, the remaining uncertainty about the marginal profitability $Var[\tilde{\mu}|y]$ dissipates and the firm's investment becomes more aggressive. Since $Var[k(y)]$ decreases monotonically in the adjustment cost z (given disclosure quality β), z measures the degree of the investment effect. When z is infinitely large, the optimal investment level is always zero and the economy becomes a pure exchange economy.

Second, the risk allocation effect of disclosure quality, similar to that in Dye (1990), is at work because the firm has m units of existing investment. Since the investment effect interacts with the risk allocation effect, I focus on the "residual" risk allocation effect by keeping the total investment fixed. In such a pure exchange economy, as defined later in Definition 1, disclosure quality does not eliminate the risk of the firm's cash flow; instead, it only allocates the risk between current and new investors. One can decompose the total risk, i.e., $Var[\tilde{\mu}]$, into two components, $Var[E[\tilde{\mu}|y]]$, and $Var[\tilde{\mu}|y]$. The first component is tantamount to the *ex ante* uncertainty of the trading price (price risk), while the second component characterizes the remaining uncertainty of the firm's cash flow (cash flow risk). Current investors bear the price risk, and new investors take the cash flow risk in return for a risk premium. Disclosure quality substitutes the price risk for the cash flow risk. Figure 2 illustrates this risk allocation effect in the pure exchange economy.

I prefer the label "risk allocation" to "risk sharing." The essence of risk sharing in the sense of Wilson (1968) is that trading reduces the total risk by diversifying the idiosyncratic risk. Optimal risk sharing requires that all investors hold the same portfolio under certain conditions. Such risk sharing exists in an model where contemporary investors trade among themselves.[4] However, in a model where one group of investors sell to another, at any level

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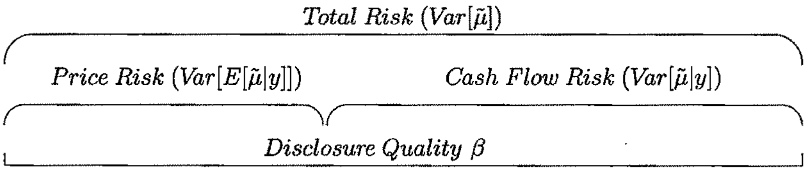
[4] For example, suppose two investors who have the same CARA utility are endowed with two risky assets, $\tilde{x}_1$ and $\tilde{x}_2$, respectively. Trading between them results in the allocation $\left(\frac{\tilde{x}_1 + \tilde{x}_2}{2}, \frac{\tilde{x}_1 + \tilde{x}_2}{2}\right)$. The total variance of this allocation is smaller than the initial sum of variance: $2Var\left[\frac{\tilde{x}_1 + \tilde{x}_2}{2}\right] = \frac{Var[\tilde{x}_1] + Var[\tilde{x}_2] + 2Cov[\tilde{x}_1, \tilde{x}_2]}{2} \leq Var[\tilde{x}_1] + Var[\tilde{x}_2]$.

FIGURE 2
The Risk Allocation Effect


Total Risk ($Var[\tilde{\mu}]$)
Price Risk ($Var[E[\tilde{\mu}|y]]$)
Cash Flow Risk ($Var[\tilde{\mu}|y]$)
Disclosure Quality β

of disclosure, the risk current investors face is independent of that new investors take. Thus, disclosure allocates the risk between current and new investors, but does not reduce total risk.

The presence of both the investment and risk allocation effects and their interaction complicate the model. While I prove the main results for the general model, I also analyze three special economies to enhance the intuition for the general results. The first two special economies, the pure exchange economy and the economy with constant return to scale (the CRTS economy), represent two extreme cases of the investment effect; the third special economy, the economy without existing investment, isolates the investment effect from the risk allocation effect.

Definition 1: *The pure exchange economy* is an economy in which the firm cannot change investment after disclosure. Mathematically, it is achieved by setting the adjustment cost of new investment z to infinity. In addition, I normalize m to one unit in this case. Thus, $\tilde{F}_{pe} = \lim_{z\to\infty, m\to 1} \tilde{F} = \mu_0 + \tilde{\mu}$. The subscript *pe* stands for pure exchange.

Definition 2: *The CRTS economy* is an economy in which the firm's new investment exhibits the property of constant return to scale. It is achieved by setting the adjustment cost of new investment z to zero. Thus, $\tilde{F}_{crts} = \lim_{z\to 0} \tilde{F} = m(\mu_0 + \tilde{\mu}) + k\tilde{\mu}$. The subscript *crts* stands for constant return to scale.

Definition 3: *The economy without existing investment* is an economy in which the firm does not have existing investment before disclosure. It is achieved by setting the existing investment level m to zero. Thus, $\tilde{F}_{we} = \lim_{m\to 0} \tilde{F} = k\tilde{\mu} - \frac{z}{2} k^2$. The subscript *we* stands for without endowment.

Finally, disclosure quality affects the *ex ante* distribution of the firm's cash flow. The firm's cash flow conditional on the disclosure y, $\tilde{F}|y$, is normally distributed and depends on the realization of the signal y. To focus on the impact of *ex ante* disclosure quality, I look at the unconditional mean and variance of the firm's cash flow before the disclosure, denoted as M and V, respectively. The unconditional stock price, denoted as P, is then a function of M and V.

$$M \equiv E[E[\tilde{F}|y]]; \tag{2}$$

$$V \equiv E[Var[\tilde{F}|y]]; \tag{3}$$

$$P \equiv E[p(y)] = M - \frac{V}{\tau_n}. \tag{4}$$

M, V, and P are taken expectations with respect to disclosure y. For simplicity, I call $E[\tilde{F}|y]$ and $Var[\tilde{F}|y]$ the conditional mean and variance of the firm's cash flow, M and V the mean and the variance of the firm's cash flow, and P the stock price, whenever there is no confusion.

Lemma 2 (Disclosure Quality and Cash Flow Distribution): As disclosure quality improves, both the mean of the firm's cash flow (M) and the stock price (P) increase, but the variance of the firm's cash flow (V) increases if and only if the adjustment cost of new investment is sufficiently low ($z < z^*$). The cutoff z^* is given in Expression (A-15) in the Appendix.

With the investment effect, disclosure quality changes both the mean and variance of the firm's cash flow, and the variance of the firm's cash flow could increase with disclosure quality. Therefore, the investment effect is important for the economic consequences of disclosure quality, given that all the main variables of interest—cost of capital, current investors' welfare, and new investors' welfare—are related to the characteristics of the entire distribution of the firm's cash flow. The importance becomes more obvious when Lemma 3 reveals that the impact of disclosure quality on the distribution of the firm's cash flow varies dramatically in three special economies.

Lemma 3 (Disclosure Quality and Cash Flow Distribution in the Special Economies): As disclosure quality improves:

1. in the pure exchange economy, the mean is constant, the variance decreases, and the price increases;
2. in the CRTS economy, the mean, the variance, and the stock price all increase;
3. in the economy without existing investment, the mean and the stock price increase, but the variance increases if and only if the adjustment cost is sufficiently low $\left(z < \frac{2}{(\beta - \alpha)\tau_n}\right)$.

Table 1 summarizes Lemma 2 and 3 and Figure 3 is a figurative illustration of Lemma 3.

III. MAIN RESULTS

Having characterized the equilibrium, I conduct comparative statics in this section to addresses the main research questions. I first identify necessary and sufficient conditions under which disclosure quality reduces cost of capital and improves the welfare of current and new investors. I then compare these conditions to show that they are not equivalent, nor do they subsume each other, as summarized in Table 2. Therefore, cost of capital does

TABLE 1
Effects of Disclosure Quality on the Mean and Variance of the Firm's Cash Flow

Economies (Cash Flow $\tilde{F}$)	Mean M	Variance V
Pure Exchange $(\mu_0 + \tilde{\mu})$	Constant	Decrease
CRTS $(m(\mu_0 + \tilde{\mu}) + k\tilde{\mu})$	Increase	Increase
No Endowment $\left(k\tilde{\mu} - \frac{z}{2}k^2\right)$	Increase	Increase/Decrease
General Economy $\left(m(\mu_0 + \tilde{\mu}) + k\tilde{\mu} - \frac{z}{2}k^2\right)$	Increase	Increase/Decrease

FIGURE 3
The Impacts of β on M and V in Three Special Economies

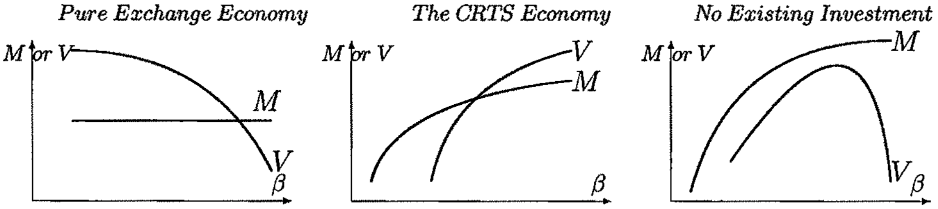


TABLE 2
Effects of Disclosure Quality on Cost of Capital and Investors' Welfare

Economies	Cost of Capital	Current Investors' Welfare	New Investors' Welfare
Pure Exchange	Decrease	Increase/Decrease	Decrease
CRTS	Increase	Increase	Increase
No Endowment	Decrease	Increase	Increase/Decrease
General Economy	Increase/Decrease*	Increase/Decrease**	Increase/Decrease***

*, **, *** Conditions differ from and do not subsume each other.

not always summarize the impact of disclosure quality on the welfare of either current or new investors.

Disclosure Quality and Cost of Capital

Cost of capital is defined as the expected return on the firm's equity. Importantly, it is the cost of capital for new investors to invest in the firm's stocks, not the cost of capital for the firm to undertake new investment.

$$E[\tilde{R}] = \frac{M - P}{P}. \tag{5}$$

Cost of capital could be characterized as either the conditional or unconditional expected returns on equity; the latter is a more appropriate characterization in this analysis because it measures cost of capital over all possible information realizations. Empirically, this unconditional expected return $E[\tilde{R}]$ is the value-weighted average of the conditional expected returns $E[\tilde{R}|y]$, representing the obtainable return for an investor who invests in the same firm over time or simultaneously in many similar firms.[5] In addition, unlike conditional expected return, $E[\tilde{R}]$ is always positive if $\mu_0 > \hat{\mu}_0 = \frac{2\alpha z m^2 - \beta\tau_n}{4\alpha m + 2\alpha^2 z m\tau_n + 2\alpha\beta z m\tau_n}$. Note that $\hat{\mu}_0$ is independent of the signal y.

Proposition 1 (Disclosure Quality and Cost of Capital): As disclosure quality improves, cost of capital decreases if and only if the adjustment cost of new investment is sufficiently high ($z > z^*$) or the prior expected profitability of the firm's existing investment is sufficiently low ($\mu_0 < \mu_0^*$). z^* is the same as that in Lemma 2, and μ_0^* is given in Expression (A-18) in the Appendix.

The intuition behind Proposition 1 centers on the impacts of disclosure quality on the characteristics of the distribution of the firm's cash flow (Lemma 2). Cost of capital measures the per-dollar risk premium. The size of the overall risk premium increases with the variance of the firm's cash flow, and the scaling variable (i.e., the stock price) increases with the prior expected profitability of the firm's existing investment. Disclosure quality could increase cost of capital if it increases the variance and the variance grows faster than the stock price. A sufficiently low adjustment cost guarantees the increasing variance and a sufficiently high prior expected profitability further ensures that the per-dollar variance is increasing. Thus, Proposition 1 extends the relation between disclosure quality and cost of capital to a production economy and confirms the conjecture in Lambert et al. (2007, 410).

Cost of capital could be rewritten as a function of the variance-mean ratio of the firm's cash flow by plugging Equation (4) into Equation (5).

$$E[\tilde{R}] = \frac{1}{\frac{\tau_n}{\frac{V}{M}} - 1}. \tag{6}$$

Cost of capital increases monotonically with the variance-mean ratio $\left(\frac{V}{M}\right)$ and decreases with new investors' risk tolerance (τ_n). Using “'” to denote the partial derivative with respect to β, cost of capital increases with disclosure quality if and only if the sign of the following partial derivative is positive.

---

[5] $E[\tilde{R}] = \frac{M - P}{P} = \int_{-\infty}^{\infty} \frac{E[\tilde{F}|y] - p(y)}{p(y)} \frac{p(y)}{\int_{-\infty}^{\infty} p(y)\phi(y)dy} \phi(y)dy = \int_{-\infty}^{\infty} E[\tilde{R}|y] \frac{p(y)}{\int_{-\infty}^{\infty} p(y)\phi(y)dy} \phi(y)dy$ where $\phi(y)$ is the probability density function of $\tilde{y}$. Note that the equal-weighted return, $E\left[\frac{E[\tilde{F}|y] - p(y)}{p(y)}\right]$, is difficult to calculate because of the appearance of the random variable y in the denominator.

$$\frac{\partial E[\tilde{R}]}{\partial \beta} = \frac{MM'\tau_n}{(\tau_n M - V)^2}\left(\frac{V'}{M'} - \frac{V}{M}\right). \tag{7}$$

When does $\frac{\partial E[\tilde{R}]}{\partial \beta} > 0$? First, a positive V' is a necessary condition for the derivative to be positive. All variables in Equation (7) are always positive except V'.[6] If $V' < 0$, then $\frac{V'}{M'} < 0$, $\frac{\partial E[\tilde{R}]}{\partial \beta} < 0$, and disclosure quality monotonically reduces cost of capital. By Lemma 2, $V' < 0$ is equivalent to the condition that the adjustment cost of the firm's new investment is sufficiently high ($z > z^*$). This explains the condition about the adjustment cost z in Proposition 1.

Second, when $V' > 0$, the sign of the derivative is determined solely by the sign of the difference between two variance-mean ratios, $\frac{V'}{M'} - \frac{V}{M}$. The economic intuition of these two ratios is as follows. Consider a marginal increase in disclosure quality that causes an incremental change in the firm's cash flow. The firm's new cash flow becomes a weighted average of the pre-change cash flow with a variance-mean ratio of $\frac{V}{M}$ and the incremental cash flow with a variance-mean ratio of $\frac{V'}{M'}$. If the variance-mean ratio of the incremental cash flow $\left(\frac{V'}{M'}\right)$ is greater than that of the pre-change cash flow $\left(\frac{V}{M}\right)$, then the new (weighted average) variance-mean ratio becomes greater and cost of capital increases.[7]

Finally, to explain the determinants of the variance-mean ratios, note that the prior expected profitability μ_0 does not change the incremental cash flow, but affects the pre-change cash flow by altering the mean M. All else equal, when μ_0 is greater, M is greater, and thus $\frac{V}{M}$ is smaller. When μ_0 is large enough, $\frac{V'}{M'}$ exceeds $\frac{V}{M}$ and $\frac{\partial E[\tilde{R}]}{\partial \beta} > 0$. This completes the intuition of Proposition 1. Further, the intuition that disclosure quality influences cost of capital through its impact on the variance-mean ratio of the firm's cash flow becomes more transparent in three special economies, as noted before.

Corollary 1 (Disclosure Quality and Cost of Capital in the Special Economies): As disclosure quality improves, cost of capital decreases in the pure exchange economy and in the economy without existing investment, but increases in the CRTS economy.

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[6] Under condition $\mu_0 > \hat{\mu}_0$, the price P is positive. Since the variance V is positive, $M = P + \frac{1}{\tau_n} V$ is also positive. Finally, Lemma 2 proves that M' is positive.

[7] Another interpretation of the result is to rewrite Equation (7) as $\frac{\partial E[\tilde{R}]}{\partial \beta} = \frac{MV\tau_n}{(\tau_n M - V)^2}\left(\frac{V'}{V} - \frac{M'}{M}\right)$. The impact of disclosure quality on cost of capital is determined by the disclosure quality elasticity of variance and mean.

In the pure exchange economy, the variance-mean ratio is $\left(\frac{V}{M}\right)_{pe} = \lim_{z \to \infty, m \to 1} \frac{V}{M} = \frac{Var[\tilde{\mu}|y]}{\mu_0}$. Disclosure quality monotonically reduces $Var[\tilde{\mu}|y]$ and thus reduces cost of capital. Disclosure quality does not change the mean but always reduces the conditional variance of the firm's cash flow, resulting in a decreasing variance-mean ratio.

When the investment effect is present, disclosure quality affects both the mean and variance of the firm's cash flow. As a result, the impact of disclosure quality on cost of capital becomes more subtle. In the economy without existing investment, the variance-mean ratio is $\left(\frac{V}{M}\right)_{we} = \lim_{m \to 0} \frac{V}{M} = \frac{2\tau_n}{4 + \frac{z\tau_n}{Var[\tilde{\mu}|y]}}$. Disclosure quality also monotonically reduces $Var[\tilde{\mu}|y]$ and thus reduces the cost of capital. In this economy, the mean of the firm's cash flow monotonically increases with disclosure quality, while the variance is not monotonic. The mean grows faster than the variance. As a result, disclosure quality also decreases the variance-mean ratio.[8]

The CRTS economy provides an example in which the variance outpaces the mean. In this economy, the variance-mean ratio is $\left(\frac{V}{M}\right)_{crts} = \lim_{z \to 0} \frac{V}{M} = \frac{\tau_n}{2 + \frac{m\mu_0\tau_n}{V_{crts}}}$. Disclosure quality monotonically increases V_{crts} and thus increases the cost of capital. As disclosure quality improves, both the mean and variance increase, but the variance grows faster than the mean, leading to an increasing variance-mean ratio.

In sum, disclosure quality affects the cost of capital through its impact on the variance-mean ratio of the firm's cash flow. In the presence of the investment effect, disclosure quality affects both the mean and variance of the firm's cash flow. As a result, there are plausible conditions under which disclosure quality increases the cost of capital.

Disclosure Quality and Current Investors' Welfare

I define investor welfare as investors' *ex ante* expected utility: the utility after the disclosure quality has been set, but before the signal comes out. In particular, current investors' welfare is as follows:

$$E[U(W_c)] = E[E[U(W_c)|y]] = H_1 \exp(H_2), \tag{8}$$

where H_1 and H_2 are given in Expressions (A-21) and (A-22) in the Appendix. The complexity of H_1 and H_2 results from the investment effect, which induces a non-normal distribution of $p(y)$.

Proposition 2 (Disclosure Quality and Current Investors' Welfare): As disclosure quality improves, current investors are better off if and only if they are sufficiently risk-tolerant relative to new investors $\left(\tau_c > \frac{\tau_n}{2}\right)$ or the adjustment cost of new investment is sufficiently low ($z < z_c^*$). The cutoff is characterized in Expression (A-23) in the Appendix.

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[8] Lambert et al. (2007) analyze a similar example of the production economy without existing investment.

Remark 1 (Cost of Capital and Current Investors' Welfare): In the analysis of the economic consequences of disclosure quality, as disclosure quality changes, cost of capital does not always move in parallel with current investors' welfare.

The intuition for Proposition 2 and Remark 1 lies in the dual effect of disclosure quality of facilitating investment and allocating risk. On one hand, disclosure better aligns the firm's investment decisions with the market's expectations, which enhances current investors' welfare. On the other hand, disclosure quality also allocates the risk between current and new investors by resolving the uncertainty before current investors transfer it to new investors. Whether this risk allocation effect improves current investors' welfare depends on the relative risk tolerance of current and new investors, as in Dye (1990). When current investors are sufficiently risk-averse and the improvement in investment is marginal, disclosure quality could reduce current investors' welfare.

The special economies provide transparent intuition. The pure exchange economy illustrates the welfare consequences of the risk allocation effect; the economy without existing investment reveals the welfare impact of the investment effect; and all three special economies are informative about the discrepancy between cost of capital and current investors' welfare in Remark 1.

Corollary 2 (Disclosure Quality and Current Investors' Welfare in the Special Economies): As disclosure quality improves, current investors are better off in the pure exchange economy if and only if they are sufficiently risk-tolerant $\left(\tau_c > \frac{\tau_n}{2}\right)$, and they are always better off in both the economy without existing investment and the CRTS economy.

In the pure exchange economy, disclosure divides the firm's risk into price risk and cash flow risk. Disclosure quality reduces cash flow risk but increases price risk, creating a trade-off for current investors' welfare. Formally, the certainty equivalent of the welfare of current investors is as follows:

$$(CE_c)_{pe} = P - \frac{1}{2\tau_c} Var[p(y)]; \tag{9}$$

$$= \mu_0 - \frac{1}{\tau_n} \underbrace{Var[\tilde{\mu}|y]}_{\text{Cash Flow Risk}} - \frac{1}{2\tau_c} \underbrace{Var[E[\tilde{\mu}|y]]}_{\text{Price Risk}}; \tag{10}$$

$$= \mu_0 - \frac{1}{2\tau_c}\frac{1}{\alpha} + \left(\frac{1}{2\tau_c} - \frac{1}{\tau_n}\right) Var[\tilde{\mu}|y]. \tag{11}$$

Equation (9) indicates that current investors care about not only the average level, but also the *ex ante* uncertainty of their *ex post* wealth $p(y)$. Equation (10) shows that current investors suffer from both cash flow risk and price risk, constituting the initial risk of the firm's cash flow ($Var[\tilde{\mu}]$). Disclosure quality substitutes the price risk for the cash flow risk. Equation (11) reveals the trade-off for current investors. Loosely speaking, one more unit of price risk costs current investors a relative disutility of $\frac{1}{2\tau_c}$ utile, and one more unit of cash flow risk costs $\frac{1}{\tau_n}$ utile. Thus, the reduction in cash flow risk improves current investors' welfare if and only if $\tau_c > \frac{\tau_n}{2}$. When it is relatively more expensive to bear the risk by themselves, current investors would rather pay new investors to assume this. In this case, by resolving uncertainty before trading, disclosure quality prevents current investors from transferring more risk to new investors and makes current investors worse off.

Having analyzed how disclosure quality affects current investors' welfare in the pure exchange economy, I now discuss the intuition behind Remark 1 that cost of capital is a complete measure of current investors' welfare only in limited circumstances. Cost of capital is synchronous with the welfare of current investors when the reduction of cost of capital arises from the reduction of new investors' risk aversion or when current investors are sufficiently risk-tolerant. But when current investors are sufficiently risk-averse and have to sell their holdings to new investors subsequent to disclosure, disclosure quality could reduce cost of capital as well as current investors' welfare.

In the pure exchange economy, cost of capital measures only the average level of current investors' *ex post* wealth, but does not capture the *ex ante* uncertainty of their wealth. Disclosure quality affects both cost of capital and the *ex ante* uncertainty of their wealth simultaneously. To see this effect, it is helpful to decompose current investors' certainty equivalent into three components:

$$(CE_c)_{pe} = \mu_0 - \frac{E[\tilde{R}]}{1 + E[\tilde{R}]}\mu_0 - \frac{1}{2\tau_c} Var[E[\tilde{\mu}|y]]. \tag{12}$$

The first component is the expected value of the firm's cash flow, the second component is the risk discount current investors concede to new investors, and the last is current investors' utility loss from price risk. One case in which cost of capital moves in parallel with current investors' welfare is when the change in cost of capital arises from the change in new investors' risk tolerance. For example, since $E[\tilde{R}] = \frac{1}{\frac{\frac{\tau_n}{V}}{M} - 1}$, an increase in new investors' risk tolerance (τ_n) reduces cost of capital $E[\tilde{R}]$ (the second component), but keeps the price risk $Var[E[\tilde{\mu}|y]]$ (the third component) unchanged. As a result, the decrease in cost of capital improves the welfare of current investors by reducing the risk premium new investors require.

However, this analysis focuses on the relation between cost of capital and investor welfare intermediated by disclosure quality. In this case, new investors' risk tolerance is not affected by disclosure quality. Lower cost of capital is associated with higher current investors' welfare only if current investors are sufficiently risk-tolerant. Lower cost of capital caused by higher disclosure quality reduces the risk premium current investors pay to new investors (the second component in Equation (12)), but the benefit comes at the cost of increased exposure to price risk (the third component in Equation (12)). When current investors are sufficiently risk-tolerant, the saving in risk premium dominates their disutility from the increased exposure to the price risk. As a result, they are better off as the cost of capital decreases. But when current investors are sufficiently risk-averse, cost of capital is no longer synchronous with current investors' welfare.

In the economy without existing investment, there is no risk before disclosure, and thus the risk allocation effect is muted. Current investors' welfare is $(E[U(W_c)])_{we} = \lim_{m \to 0} E[U(W_c)] = -\frac{1}{\sqrt{1 + \frac{2}{\tau_c} P_{we}}}$. Disclosure quality affects current investors' welfare only through its impact on the level of their wealth. Since disclosure quality monotonically increases the stock price, the investment effect enhances current investors' welfare. Furthermore, cost of capital is not a comprehensive measure of the impact of disclosure quality on the level of current investors' wealth. Since $P = \frac{M}{1 + E[\tilde{R}]}$, disclosure quality improves the mean and reduces cost of capital at the same time. Therefore, cost of capital is not the only channel for disclosure quality to influence current investors' welfare. This observation substantiates Remark 1.

In the CRTS economy, the risk allocation is also absent because the firm could always undo the existing investment: $k(y) = -m + \frac{\tau_n}{2} \frac{E[\tilde{\mu}|y]}{Var[\tilde{\mu}|y]}$. Current investors' welfare is $(E[U(W_c)])_{crts} = \lim_{z \to 0} E[U(W_c)] = -\frac{\exp\left(-\frac{m\mu_0}{\tau_c}\right)}{\sqrt{1 + \frac{2(P_{crts} - m\mu_0)}{\tau_c}}}$. Therefore, current investors care only about the level of their wealth P, implying that current investors benefit from better disclosure. Moreover, Remark 1 is more evident in the CRTS economy because both cost of capital and current investors' welfare increase with disclosure quality. While disclosure quality increases cost of capital, it also improves the mean of the firm's cash flow. The welfare gain from the increased mean dominates the welfare loss from the increased cost of capital, leading to the overall improved welfare for current investors.

In summary, current investors care about both the level and the *ex ante* uncertainty of their wealth. Cost of capital does not capture the *ex ante* uncertainty and is only associated with one component of the level of current investors' wealth. Disclosure quality could affect cost of capital and other components of current investors' welfare simultaneously. Therefore, only in some limited cases does cost of capital move in parallel with current investors' welfare as disclosure quality changes. Beyond these cases, cost of capital becomes less effective as a measure of current investors' welfare.

Disclosure Quality and New Investors' Welfare

Similarly, the welfare of new investors is their *ex ante* expected utility:

$$\begin{aligned} E[U(W_n)] &= -E[E[U(W_n)|y]] \\ &= -E\left[\exp\left(-\frac{1}{2\tau_n^2} Var[\tilde{F}|y]\right)\right] = N_1 \exp(N_2), \end{aligned} \tag{13}$$

where N_1 and N_2 are given in Expressions (A-26) and (A-27) in the Appendix.

Proposition 3 (Disclosure Quality and New Investors' Welfare): As disclosure quality improves, new investors are better off if and only if one of the following two conditions holds:

1. If the initial disclosure quality is low ($\beta < \alpha$), then either the adjustment cost of new investment is sufficiently low ($z < z_n^*$) or the level of existing investment is sufficiently low ($m < m_n^*$);
2. If the initial disclosure quality is high ($\beta > \alpha$), then either the adjustment cost of new investment is sufficiently low ($z < z_n^*$), or the adjustment cost of new investment is modest (i.e., $z_n^* < z < z_n^{**}$) and the level of existing investment is sufficiently low ($m < m_n^*$).

Proposition 3 is illustrated in Figure 4. Disclosure quality increases new investors' welfare in the grid areas. The cut-offs, z_n^*, z_n^{**}, and m_n^*, are given in Expressions (A-28), (A-29), and (A-30) in the Appendix.

Remark 2 (Cost of Capital and New Investors' Welfare): In the analysis of the economic consequences of disclosure quality, as disclosure quality changes, cost of capital does not always move in parallel with new investors' welfare.

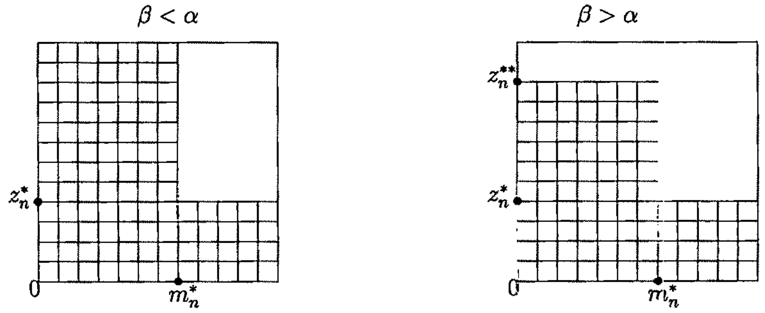
Remark 3 (The Tension between Current and New Investors): Current and new investors could have conflicting demands for disclosure quality.

There are two elements in the intuition behind Proposition 3. First, new investors' conditional expected utility increases with the cash flow risk ($Var[\tilde{F} \cdot y]$). Second, they are averse to the *ex ante* uncertainty of their conditional expected utility.

The first element may seem surprising, although it is evident in Equation (13) that $E[U(W_n)]$ increases with $Var[\tilde{F}|y]$. Not only do new investors gain positive surplus from trading in a competitive market, but also the size of the surplus increases with the cash flow risk they bear. The following analysis reveals that the source of new investors' trading surplus is their risk aversion. Further, the cash flow risk increases their surplus because it makes their demand less price sensitive and reduces competition among themselves.

Since new investors are risk-averse, their demand (D) is downward-sloping, as shown in the Appendix (Equation (A-3)):

FIGURE 4
The Impact of Disclosure Quality on New Investors' Welfare



$$D = \tau_n \frac{E[\tilde{F}|y] - p(y)}{Var[\tilde{F}|y]}. \tag{14}$$

The downward-sloping demand yields diminishing marginal utility $\left(\mathrm{MU} = E[\tilde{F}|y] - \frac{Var[\tilde{F}|y]}{\tau_n} D\right)$, which in turn determines the price ($p(y)$) in equilibrium. As a result, the price is lower than the average utility $\left(\mathrm{AU} = E[\tilde{F}|y] - \frac{Var[\tilde{F}|y]}{2\tau_n} D\right)$. The gap between MU and AU, $\frac{Var[\tilde{F}|y]}{2\tau_n} D$, is the source of new investors' surplus from trading in a competitive market. In contrast, when new investors become risk-neutral in the limit, the gap between MU and AU dissipates and $p(y) = E[\tilde{F}|y]$. As a result, new investors always pay a "fair" price for whatever they get, consistent with the notion that trading is a fair game for *risk-neutral* investors in a competitive market. New investors' welfare is not directly affected by whatever impacts disclosure has on the mean of the future cash flow (M) because the price adjustment absorbs such impacts.

Competition among new investors does affect and reduce the surplus they gain from trading, but in a subtle way. When the cash flow risk ($Var[\tilde{F}|y]$) is higher, new investors' demand for the firm's shares in Equation (14) becomes less sensitive to price. That is, the slope of the demand curve $\left(-\frac{Var[\tilde{F}|y]}{\tau_n}\right)$ becomes much steeper. Less sensitive demand amounts to less competition among new investors, increasing the gap between MU and AU and thus the surplus accruing to new investors.

The forced nature of current investors' sales increases, but does not generate, new investors' surplus. When current investors have to leave the market after disclosure, the total risk-taking capacity in the market shrinks, reducing the overall competition for the firm's shares and improving the reward for risk-taking by new investors. However, even if current investors stay in the market, new investors still gain surplus from trading, although both the gap between MU and AU and the equilibrium holding of new investors become smaller, leading to lower surplus for new investors.

Second, new investors' welfare is the probability-weighted average of their conditional expected utility. Since they are risk-averse, new investors are averse to the *ex ante* uncertainty of their conditional expected utility. From the perspective of new investors before disclosure, one realization of y leads to one conditional distribution of the firm's cash flow ($\tilde{F}|y$) with differential mean and variance. Thus, disclosure creates *ex ante* uncertainty of the conditional cash flow risk ($Var[\tilde{F}|y]$) and the *ex ante* uncertainty of new investors' conditional expected utility. The special economies make the intuition more transparent and illustrate Remarks 2 and 3.

Corollary 3 (Disclosure Quality and New Investors' Welfare in the Special Economies): As disclosure quality improves, new investors are always worse off in the pure exchange economy, always better off in the CRTS economy, and better off in the economy without existing investment if and only if the adjustment cost of new investment is sufficiently low $\left(z < \frac{2}{(\beta - \alpha)\tau_n}\right)$.

In the pure exchange economy, new investors' welfare is $(E[U(W_n)])_{pe} = \lim_{z \to \infty, m \to 1} E[U(W_n)] = -\exp\left(-\frac{1}{2t_n^2} Var[\tilde{\mu}|y]\right)$. Disclosure quality decreases new investors' welfare by leaving less risk for them to assume through trading. Moreover, cost of capital does not link disclosure quality to new investors' welfare, either. High disclosure quality reduces cost of capital by materializing more risk before trading, but this early resolution of uncertainty reduces the demand for new investors' risk tolerance and thus reduces their welfare. Therefore, cost of capital is a comprehensive measure of the welfare of neither current nor new investors. Finally, Remark 3 is also evident in the pure exchange economy. While current investors' preference for disclosure quality depends on their relative risk tolerance, new investors always prefer lower disclosure quality.

In the economy without existing investment, new investors' welfare increases monotonically with the variance of the firm's cash flow because $(E[U(W_n)])_{we} = \lim_{m \to 0} E[U(W_n)] = -\frac{1}{\sqrt{1 + \frac{1}{\tau_n^2} V_{we}}}$. Since the variance of the firm's cash flow is not monotonic, new investors benefit from better disclosure quality when the variance is increasing, that is, when $z < \frac{2}{(\beta - \alpha)\tau_n}$, according to Lemma 3. Moreover, this economy provides another example of the discrepancy between cost of capital and new investors' welfare. Driven by the decreasing variance-mean ratio, cost of capital decreases with disclosure quality. In contrast, new investors' welfare is driven by the variance alone, and thus could either increase or decrease with disclosure quality. Finally, this economy also lends support to Remark 3. When $z > \frac{2}{(\beta - \alpha)\tau_n}$, current investors favor more transparency while new investors prefer less.

In the CRTS economy, new investors' welfare also increases with the variance of the firm's cash flow because $(E[U(W_n)])_{crts} = \lim_{z \to 0} E[U(W_n)] = -\frac{1}{\sqrt{1 + \frac{1}{\tau_n^2} V_{crts}}}$. Since the variance of cash flow increases with disclosure quality in the CRTS economy, new investors are better off with better disclosure quality. Moreover, Remark 2 is striking in the CRTS economy. New investors' welfare increases with disclosure quality. So does the cost of capital. One can rewrite new investors' welfare as a function of cost of capital: $(E[U(W_n)])_{crts} = -\frac{1}{\sqrt{1 + \frac{m\mu_0}{\tau_n} \frac{E[\tilde{R}]}{1 - E[\tilde{R}]}}}$. Since $E[\tilde{R}] \in (0,1)$, current investors' welfare *increases* with cost of capital. Disclosure quality increases both the mean and variance of the firm's cash flow, but the variance outpaces the mean. As a result, as disclosure quality improves, the cost of capital increases because of the increasing variance-mean ratio, and new investors' welfare also increases because of the increasing variance. Thus, disclosure quality does not summarize the impact of disclosure quality on new investors' welfare. Finally, although disclosure quality increases the welfare of both current and new investors, it does so through different channels. Current investors benefit from the improved mean of the firm's cash flow but suffer from the accompanying increase in the variance, whereas new investors benefit only from the increase in the variance.

In all three special economies, new investors' preference for disclosure quality can be summarized by the impact of disclosure quality on the average level of their conditional expected utility (or the variance of the firm's cash flow V). When the risk allocation effect interacts with the investment effect, the *ex ante* uncertainty of the cash flow risk ($Var[\tilde{F}|y]$) also plays a role in new investors' welfare.

In sum, new investors earn surplus from risk-bearing in equilibrium. Their welfare is determined by both the average level and the *ex ante* uncertainty of the amount of the cash flow risk they could absorb from trading, and cost of capital is only partially associated with the first component. Since disclosure quality changes both components of the welfare of new investors, the cost of capital does not always move in concert with new investors' welfare as disclosure quality changes.

IV. DISCUSSION AND RELATED LITERATURES

This section discusses two key assumptions of the model, perfect competition and forced divestiture, in the context of the related literatures. My study links the cost of capital, an important empirical construct in accounting, to the broad economic literature on the efficiency of disclosure quality. A central result about the role of information in a capital market is that the combination of pure exchange, constant absolute risk aversion (CARA), and perfect competition leaves little room for studying investor welfare.[9] Subsequent research introduces private information acquisition (e.g., Diamond 1985), relaxes the assumption of perfect competition (e.g., Kyle 1985; Diamond and Verrecchia 1991), or incorporates production use of information (e.g., Kunkel 1982; Christensen and Feltham 1988; Pae 1999, 2002). This study examines the effect of disclosure on a firm's investment decisions.

The focus on the investment effect of disclosure entails the first key assumption: *perfect competition* among investors. My model is couched in a setting in which trade in a firm's shares is perfectly competitive. In a perfect competition setting, each individual investor is assumed to conjecture that his/her demand has no effect on price, and in equilibrium this conjecture is sustained. Because markets are perfectly liquid by construction, (il)liquidity issues never arise and adverse selection is not priced. Alternatively, in models of *im*perfect competition, disclosure serves to reduce the cost of capital and potentially enhance welfare by improving liquidity and ameliorating adverse selection (e.g., Diamond and Verrecchia 1991). A recent study by Lambert and Verrecchia (2009) explores this reasoning further. They decompose the cost of capital into three additive risk factors: market risk, (pure) liquidity risk, and adverse selection risk arising from the imperfect competition among investors. It will be interesting to see how the welfare consequences of disclosure quality change in their imperfect competition setting.

The investment effect of disclosure in my analysis draws heavily on the research about the real effects of disclosure in capital market. A firm's disclosure influences investors' perceptions which in turn guide the firm's real decisions, and both the investors' valuation and the firm's real decisions are consistently determined in a rational expectations equilibrium. This notion, developed by Kanodia (1980), has been used to study the effect of periodical performance reports (e.g., Kanodia and Lee 1998), measuring intangibles (e.g., Kanodia et al. 2004), and accounting for derivatives (e.g., Melumad et al. 1999; Kanodia et al. 2000; Sapra 2002; Sapra and Shin 2007). While these studies typically focus on the firm's *ex post* disclosure choices, I examine the firm's *ex ante* disclosure policy and thus abstract from the signaling and communication game.

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[9] Interested readers are referred to Hirshleifer (1971), Hakansson et al. (1982), Ross (1989), Dye (2001), Verrecchia (2001), Christensen et al. (2008), and Lambert and Verrecchia (2009). Verrecchia (1982) surveys the early literature. See also Holmstrom and Tirole (1993) and Dow and Rahi (2003) for the concern of the adverse welfare effect of public information in models in economics and finance.

The focus on *ex ante* disclosure quality entails the second key assumption: *forced divestiture*. In this model, current investors *have to* sell all of their shares of the firm after disclosure. While liquidity-motivated trade is a common feature of models that involve information and trade, its role in this analysis is less benign than that of simply ensuring that market-clearing prices are not fully revealing (e.g., Grossman and Stiglitz 1980; Diamond and Verrecchia 1981). By virtue of the fact that current investors are required to divest their shares after disclosure, it is in their best interest to make an investment choice subsequent to disclosure that maximizes the firm share price that is determined by the preference and belief of *new* investors. Thus, the investment choice subsequent to disclosure is not necessarily the one that current investors would prefer if they had the opportunity to retain their ownership in the firm. This modeling choice apparently contributes to the discrepancy between disclosure quality and investor welfare.

If current investors keep an exogenous portion of their holdings after disclosure, then the investment decision will reflect the weighted average of the preferences of current and new investors. This alternative does not qualitatively affect the main conclusions of the analysis due to the continuity of the results. However, if current investors (or the firm on their behalf) and new investors have differential access to information, then relaxing the assumption of forced sale complicates the model because the current investors' divestiture decisions and/or the firm's investment choice could potentially convey information. In this case, the effects of disclosure on cost of capital and investor welfare could differ from that in my model.

Finally, by studying the investment effect of disclosure quality, this study contributes to the large literature that has focused on the relation between cost of capital and disclosure quality in a pure exchange economy (e.g., Easley and O'Hara 2004; Yee 2006; Hughes et al. 2007; Lambert et al. 2007, 2008). A common theme in the previous literature is that disclosure reduces cost of capital by reducing the conditional variance (or covariance) of the firm's future cash flow because the mean of the firm's cash flow is fixed. One exception is Lambert et al. (2007), who also study the indirect effect of disclosure. They point out that cost of capital may increase with disclosure quality if disclosure changes both the mean and variance of the firm's cash flow. However, they do not link this result directly to disclosure quality. Building on their insight, I study the investment effect and identify conditions for both a positive and negative relation between disclosure quality and cost of capital. Another literature starting from Lucas (1978) approaches the topic through the connection between information and investors' intertemporal consumption decision (e.g., Veronesi 2000; Yee 2007). Instead, my study focuses on the firm's investment decisions.

V. EMPIRICAL IMPLICATIONS

With the caveat that cost of capital is not always synchronous with investor welfare, the current results provide one potential theoretical explanation for the mixed empirical evidence on the relation between disclosure quality and cost of capital. A typical empirical specification for testing the relation between disclosure quality and cost of capital is as follows:

$$E[\tilde{R}_i] = \phi X_i + \gamma \beta_i + \varepsilon_i,$$

where X_i is a vector of known risk characteristics and β_i is a proxy of a firm's (or country's) disclosure quality. The sign of γ is the variable of interest. Due to the nonlinearity of the impact of z and μ_0 on the relation between disclosure quality and cost of capital, one cannot directly interact these terms with β_i in the above regression. One way to sort out the sign of γ is to run the regression in the subsamples sorted by z and μ_0. My model predicts that the sign of γ is negative except for the subsample of low z and high μ_0.

My model also predicts the determinants of a firm's choice of *ex ante* disclosure quality. For example, according to the "bonding hypothesis" (e.g., Coffee 2002; Stulz 1999), a firm can cross-list to commit itself to higher disclosure quality. The current model predicts that firms with lower adjustment cost z and relatively risk-tolerant existing investors, such as growth firms, are more likely to seek cross-listing to maximize their existing investors' welfare. Moreover, the cross-listing firms on average receive high valuation. Other ways for a firm to commit include the firm's choices of going public or staying private and of what accounting standards and policies firms choose. The same predictions about cross-listing could also be applied directly to these corporate choices.

Finally, theoretical research has adopted many definitions of cost of capital that correspond to distinct empirical measurements. First, cost of capital in my model, as well as in Lambert et al. (2007) and Hughes et al. (2007), is derived from the price obtained from trading after the firm reveals its disclosure, while Diamond and Verrecchia (1991) and Christensen et al. (2008) examine some transformation of the price before disclosure but after the disclosure quality has been set. Thus, the former set of assumptions corresponds to an association study, while the latter is more suitable for an event study. Second, the first group of studies transform the post-disclosure price in different ways to generate construct of cost of capital. My study uses the weighted average of the conditional returns ($E[\tilde{R}]$), Hughes et al. (2007) adopt the equal weighted unscaled risk premium, and Lambert et al. (2007) draw inferences directly from conditional returns ($E[\tilde{R}|y]$).

VI. CONCLUSION

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This study examines the notion that disclosure quality improves investor welfare by reducing the cost of capital. I model a production economy in which disclosure changes a firm's investment. I identify necessary and sufficient conditions under which disclosure quality reduces cost of capital and improves the welfare of current and new investors. I then show that these conditions are neither equivalent nor do they subsume each other. Therefore, cost of capital does not summarize the impact of disclosure quality on the welfare of either current or new investors. In addition, disclosure quality creates a tension between current and new investors.

With the caveat that they are derived under the assumption of perfect competition, these results may help reconcile the mixed empirical evidence on the relation between disclosure quality and cost of capital, inform empirical efforts to measure the economic consequences of accounting disclosure, and add to the ongoing debate on the reform of financial reporting and disclosure regulation.

APPENDIX

The following proofs involve Lemma 4 and 5. They are proved first.

Lemma 4: $f(\cdot)$ is a continuous function, $f^{-1}(\cdot)$ is the inverse function of $f(\cdot)$, and $f'(\cdot)$ is the first derivative of $f(\cdot)$. If $f'(x) > 0(<0)$, then $y > f(x) \Leftrightarrow x < (>) f^{-1}(y)$.

Proof of Lemma 4

$$y > f(x) \Leftrightarrow y - f(x) > 0$$
$$\Leftrightarrow f(f^{-1}(y)) - f(x) > 0.$$

By the Lagrange Mean Value theorem, there exists $x_0 \in (x, f^{-1}(y))$ if $x < f^{-1}(y)$, or $x_0 \in (f^{-1}(y), x)$ if $x > f^{-1}(y)$ such that:

$$f(f^{-1}(y)) - f(x) = f'(x_0)(f^{-1}(y) - x) > 0.$$

Since $f'(x_0) > (<)0$, thus:

$$f^{-1}(y) - x > (<)0 \Leftrightarrow x < (>)f^{-1}(y). \qquad \blacksquare$$

Lemma 5: Suppose x is a normally distributed random variable with mean zero and variance σ^2. If $1 - 2a\sigma^2 > 0$, then:

$$E[\exp(ax^2 + bx + c)] = \frac{1}{\sqrt{1 - 2a\sigma^2}} \exp\left(\frac{\frac{1}{2}b^2\sigma^2}{1 - 2a\sigma^2} + c\right).$$

Proof of Lemma 5

$$E[\exp(ax^2 + bx + c)] = \frac{1}{\sqrt{2\pi\sigma^2}} \int_{-\infty}^{\infty} \exp(ax^2 + bx + c)\exp\left(-\frac{1}{2}\frac{x^2}{\sigma^2}\right) dx$$

$$= \frac{1}{\sqrt{2\pi\sigma^2}} \int_{-\infty}^{\infty} \exp\left(-\frac{1 - 2a\sigma^2}{2\sigma^2}\left(x^2 - 2\frac{b\sigma^2}{1 - 2a\sigma^2}x\right) + c\right) dx$$

$$= \frac{1}{\sqrt{2\pi\sigma^2}} \int_{-\infty}^{\infty} \exp\left(-\frac{1}{2}\frac{\left(x - \frac{b\sigma^2}{1 - 2a\sigma^2}\right)^2}{\frac{\sigma^2}{(1 - 2a\sigma^2)}}\right) \exp\left(\frac{\frac{1}{2}b^2\sigma^2}{1 - 2a\sigma^2} + c\right) dx$$

$$= \frac{1}{\sqrt{1 - 2a\sigma^2}} \exp\left(\frac{\frac{1}{2}b^2\sigma^2}{1 - 2a\sigma^2} + c\right) \int_{-\infty}^{\infty} \frac{1}{\sqrt{\frac{2\pi\sigma^2}{(1 - 2a\sigma^2)}}} \exp$$

$$\times \left(-\frac{1}{2}\frac{\left(x - \frac{b\sigma^2}{1 - 2a\sigma^2}\right)^2}{\frac{\sigma^2}{(1 - 2a\sigma^2)}}\right) dx$$

$$= \frac{1}{\sqrt{1 - 2a\sigma^2}} \exp\left(\frac{\frac{1}{2}b^2\sigma^2}{1 - 2a\sigma^2} + c\right). \qquad \blacksquare$$

Proof of Lemma 1

First, one solves for the price $p(y)$, given new investors' conjecture about the firm's investment decision $k(y)$. Given their knowledge of β and y, and their conjecture $k(y)$, new investors perceive that the firm's cash flow has a normal distribution with mean $E[\tilde{F}|(y,k(y))]$ and $Var[\tilde{F}|(y,k(y))]$:

$$E[\tilde{F}|(y,k(y))] = m(\mu_0 + E[\tilde{\mu}|y]) + kE[\tilde{\mu}|y] - \frac{z}{2}k^2; \tag{A-1}$$

$$Var[\tilde{F}|(y,k(y))] = (m + k)^2 Var[\tilde{\mu}|y]. \tag{A-2}$$

A representative new investor i chooses her demand D_i to maximize her expected utility by solving the following program $(P1)$:

$$\begin{aligned}(P1)\ &\max_{D_i} E[U(W_n)|(y,k(y))]\\ &= -E\left[\exp\left(-\frac{1}{\tau_n}D_i(\tilde{F}-p)\right)|(y,k(y))\right]\\ &= -\exp\left(-\frac{D_i}{\tau_n}\left(E[\tilde{F}|(y,k(y))] - p(y) - \frac{D_i}{2\tau_n}Var[\tilde{F}|(y,k(y))]\right)\right).\end{aligned}$$

The first-order condition gives the optimal demand function:

$$D_i^* = \tau_n \frac{E[\tilde{F}|(y,k(y))] - p(y)}{Var[\tilde{F}|(y,k(y))]}. \tag{A-3}$$

The second-order condition is equivalent to $-\dfrac{Var[\tilde{F}|(y,k(y))]}{\tau_n} < 0$, guaranteeing that D_i^* is the maximum solution.

Since the per capita supply of shares is normalized to one unit, market clearing requires that:

$$1 = \int_0^1 D_i^* di = \tau_n \frac{E[\tilde{F}|(y,k(y))] - p(y)}{Var[\tilde{F}|(y,k(y))]}.$$

Thus, one gets the trading price:

$$p(y) = E[\tilde{F}|(y,k(y))] - \frac{1}{\tau_n}Var[\tilde{F}|(y,k(y))]. \tag{A-4}$$

Second, anticipating the trading price $p(y)$, the firm makes the investment decision $k(y)$ to maximize the stock price by solving the following program.

$$\begin{aligned}(P2)\ \max_{k(y)} E[p(y)|y] &= E[\tilde{F}|(y,k(y))] - \frac{1}{\tau_n}Var[\tilde{F}|(y,k(y))]\\ &= m\mu_0 + (m + k(y))E[\tilde{\mu}|y] - \frac{z}{2}k^2(y) - \frac{(m + k(y))^2 Var[\tilde{\mu}|y]}{\tau_n}.\end{aligned}$$

The first-order condition gives the optimal new investment function:

$$k(y) = \frac{E[\tilde{\mu}|y]}{z + \dfrac{2}{\tau_n} Var[\tilde{\mu}|y]} - \frac{\dfrac{2}{\tau_n} Var[\tilde{\mu}|y]}{z + \dfrac{2}{\tau_n} Var[\tilde{\mu}|y]} m. \tag{A-5}$$

The second-order condition is equivalent to $-z - \dfrac{2Var[\tilde{\mu}|y]}{\tau_n} < 0$, guaranteeing that $k(y)$ is the maximum solution.

Therefore:

$$p(y) = E[\tilde{F}|y] - \frac{1}{\tau_n} Var[\tilde{F}|y]; \tag{A-6}$$

$$\begin{aligned} E[\tilde{F}|y] = m\mu_0 &- \frac{2zm^2 Var[\tilde{\mu}|y]^2}{(z\tau_n + 2Var[\tilde{\mu}|y])^2} + \frac{zm\tau_n(z\tau_n + 4Var[\tilde{\mu}|y])}{(z\tau_n + 2Var[\tilde{\mu}|y])^2} E[\tilde{\mu}|y] \\ &+ \frac{\tau_n(z\tau_n + 4Var[\tilde{\mu}|y])}{2(z\tau_n + 2Var[\tilde{\mu}|y])^2} E[\tilde{\mu}|y]^2; \end{aligned} \tag{A-7}$$

$$\begin{aligned} Var[\tilde{F}|y] = &\frac{z^2 m^2 \tau_n^2 Var[\tilde{\mu}|y]}{(z\tau_n + 2Var[\tilde{\mu}|y])^2} + \frac{2zm\tau_n^2 Var[\tilde{\mu}|y]}{(z\tau_n + 2Var[\tilde{\mu}|y])^2} E[\tilde{\mu}|y] \\ &+ \frac{\tau_n^2 Var[\tilde{\mu}|y]}{(z\tau_n + 2Var[\tilde{\mu}|y])^2} E[\tilde{\mu}|y]^2. \end{aligned} \tag{A-8}$$

■

Proof of Lemma 2

The expected mean, variance, and price are:

$$M \equiv E[E[\tilde{F}|y]] = m\mu_0 - \frac{4\alpha z m^2 + \beta\tau_n(4 + (\alpha + \beta)z\tau_n)}{2\alpha(2 + z\tau_n(\alpha + \beta))^2}; \tag{A-9}$$

$$V \equiv E[Var[\tilde{F}|y]] = \frac{(\beta + \alpha^2 z^2 m^2 + \alpha\beta z^2 m^2)\tau_n^2}{\alpha(2 + z\tau_n(\alpha + \beta))^2}; \tag{A-10}$$

$$P \equiv E\left[E[\tilde{F}|y] - \frac{1}{\tau_n} Var[\tilde{F}|y]\right] = M - \frac{1}{\tau_n} V. \tag{A-11}$$

$$\frac{\partial M}{\partial \beta} = \frac{\tau_n(8 + \alpha z(8zm^2 + 6\tau_n + \beta z\tau_n^2))}{2\alpha(2 + \alpha z\tau_n + \beta z\tau_n)^3} > 0;$$

$$\frac{\partial P}{\partial \beta} = \frac{\tau_n(2 + \alpha z(2zm^2 + \tau_n))}{2\alpha(2 + \alpha z\tau_n + \beta z\tau_n)^2} > 0.$$

By taking the partial derivative of Equation (A-10) with respect to β and solving for $\frac{\partial V}{\partial \beta} > 0$, one gets the solution $\{\alpha < \alpha^*(z), \beta < \beta^*(z)\}$, where:

$$\alpha^*(z) = \frac{2zm^2 + \tau_n}{2z^2m^2\tau_n} + \frac{1}{2}\sqrt{\frac{4z^2m^4 + 12zm^2\tau_n + \tau_n^2}{z^4m^4\tau_n^2}}; \tag{A-12}$$

$$\beta^*(z) = \frac{2 + 2\alpha z^2m^2 + \alpha z\tau_n - \alpha^2z^3m^2\tau_n}{z\tau_n + \alpha z^3m^2\tau_n}. \tag{A-13}$$

Both the cutoffs $\alpha^*(z)$ and $\beta^*(z)$ are decreasing in z, because:

$$\frac{\partial \alpha^*(z)}{\partial z} = -\frac{1}{z^3m^2\tau_n}\left(zm^2 + \tau_n + \frac{(2z^2m^4 + 9zm^2\tau_n + \tau_n^2)}{\sqrt{4z^2m^4 + 12zm^2\tau_n + \tau_n^2}}\right) < 0;$$
$$\frac{\partial \beta^*(z)}{\partial z} = -\frac{2(1 + 2\alpha z^2m^2 + \alpha^2z^3m^2(zm^2 + 2\tau_n))}{\tau_n(z + \alpha z^3m^2)^2} < 0.$$

Thus, by Lemma 4:

$$\begin{aligned}\{\alpha < \alpha^*(z), \beta < \beta^*(z)\} &\Leftrightarrow \{z < z_\alpha(\alpha), z < z_\beta(\beta)\}\\ &\Leftrightarrow z < z^*\end{aligned} \tag{A-14}$$

where $z_\alpha(\alpha)$ and $z_\beta(\beta)$ are the inverse functions of $\alpha^*(z)$ and $\beta^*(z)$ evaluated at α and β, respectively, and:

$$z^* = \min(z_\alpha(\alpha), z_\beta(\beta)). \tag{A-15}$$

Therefore, $\frac{\partial M}{\partial \beta} > 0, \frac{\partial P}{\partial \beta} > 0, \frac{\partial V}{\partial \beta} > 0 \Leftrightarrow \{z > z^*\}$. ■

Proof of Lemma 3

Lemma 3 is proved by taking the limit ($z \to \infty$, $z \to 0$, $m \to 0$) of the conditions in Lemma 2. ■

Proof of Proposition 1

Cost of capital is defined as:

$$E[\tilde{R}] = \frac{M - P}{P}. \tag{A-16}$$

By Equations (A-9), (A-10), and (A-11):

$$E[\tilde{R}] = \frac{2(\beta + \alpha^2z^2m^2 + \alpha\beta z^2m^2)\tau_n}{(2 + \alpha z\tau_n + \beta z\tau_n)(\beta\tau_n + 2\alpha^2zm\tau_n\mu_0 + 2\alpha m(2\mu_0 + \beta z\tau_n\mu_0 - zm))}. \tag{A-17}$$

By taking the partial derivative of Equation (A-17) with respect to β, and solving for $\frac{\partial E[\tilde{R}]}{\partial \beta} > 0$ with respect to $\{\alpha, \beta, \mu_0\}$, one obtains the solution $\{\alpha < \alpha^*(z), \beta < \beta^*(z), \mu_0 > \mu_0^*\}$, where $\alpha^*(z)$ and $\beta^*(z)$ are the same as in Expressions (A-12) and (A-13), and:

$$\mu_0^* = \frac{z(\beta^2\tau_n^2 + \alpha m^2(4 + 4\alpha z(zm^2 + \tau_n) + (\alpha + \beta)^2 z^2 \tau_n^2))}{2\alpha m(2 + \alpha z\tau_n + \beta z\tau_n)(2 + (\alpha - \beta)z\tau_n + \alpha z^2 m^2(2 - (\alpha + \beta)z\tau_n))}. \qquad \text{(A-18)}$$

According to the proof of Lemma 2:

$$\{\alpha < \alpha^*(z), \beta < \beta^*(z), \mu_0 > \mu_0^*\} \Leftrightarrow \{z < z^*, \mu_0 > \mu_0^*\}.$$

Therefore, $\frac{\partial E[\tilde{R}]}{\partial \beta} < 0 \Leftrightarrow \{\{z > z^*\}, \{\mu_0 < \mu_0^*\}\}$. ■

Proof of Corollary 1

Corollary 1 is proved by taking the limit ($z \rightarrow \infty$, $z \rightarrow 0$, $m \rightarrow 0$) of the conditions in Proposition 1. ■

Proof of Proposition 2

Current investors' welfare is

$$E[U(W_c)] = E[E[U(W_c)|y]]$$

$$= -E\left[E\left[\exp\left(-\frac{1}{\tau_c}p\right)|y\right]\right]; \qquad \text{(A-19)}$$

$$= H_1 \exp(H_2); \qquad \text{(A-20)}$$

$$H_1 = \frac{1}{\sqrt{1 + \frac{\beta\tau_n}{\alpha\tau_c(2 + \alpha z\tau_n + \beta z\tau_n)}}}; \qquad \text{(A-21)}$$

$$H_2 = \frac{m^2(2\alpha z\tau_c + \beta z\tau_n)}{2\tau_c(2\alpha\tau_c + \beta\tau_n + \alpha^2 z\tau_c\tau_n + \alpha\beta z\tau_c\tau_n)} - \frac{m\mu_0}{\tau_c}. \qquad \text{(A-22)}$$

The step from Equation (A-19) to Equation (A-20) involves Lemma 5. By taking the partial derivative of Equation (A-20) with respect to β and solving for $\frac{\partial E[U(W_c)]}{\partial \beta} > 0$, one gets $\left\{\left\{\tau_c \geq \frac{\tau_n}{2}\right\}, \left\{\tau_c < \frac{\tau_n}{2}, m < m_c^*(z)\right\}\right\}$, where:

$$m_c^*(z) = \sqrt{\frac{(2 + \alpha z\tau_n)(2\alpha\tau_c + \beta\tau_n + \alpha^2 z\tau_c\tau_n + \alpha\beta z\tau_c\tau_n)}{\alpha^2 z^2(\tau_n - 2\tau_c)(2 + \alpha z\tau_n + \beta c\tau_n)}}.$$

When $\tau_c < \frac{\tau_n}{2}$, $m_c^*(z)$ decreases in adjustment cost z because:

$$\frac{\partial m_c^*(z)}{\partial z} = -H\sqrt{\frac{\alpha^2 z^2(\tau_n - 2\tau_c)(2 + \alpha z\tau_n + \beta c\tau_n)}{(2 + \alpha z\tau_n)(2\alpha\tau_c + \beta\tau_n + \alpha^2 z\tau_c\tau_n + \alpha\beta z\tau_c\tau_n)}} < 0$$

where H is a positive constant when $\tau_c < \frac{\tau_n}{2}$. By Lemma 4 and denoting z_c^* as the inverse function of $m_c^*(z)$ evaluated at m, one has:

$$m < m_c^*(z) \Leftrightarrow z < z_c^*. \tag{A-23}$$

Therefore, $\frac{\partial E[U(W_c)]}{\partial \beta} > 0 \Leftrightarrow \left\{\left\{\tau_c \geq \frac{\tau_n}{2}\right\}, \left\{\tau_c < \frac{\tau_n}{2}, z < z_c^*\right\}\right\}.$ ■

Proof of Corollary 2

Corollary 2 is proved by taking the limit ($z \rightarrow \infty$, $z \rightarrow 0$, $m \rightarrow 0$) of the conditions in Proposition 2. ■

Proof of Proposition 3

New investors' welfare is:

$$\begin{aligned} E[U(W_n)] &= E[E[U(W_n)|y]] \\ &= -E\left[E\left[\exp\left(-\frac{1}{\tau_n}(\tilde{F} - p)\right)|y\right]\right]; && \text{(A-24)} \\ &= N_1\exp(N_2); && \text{(A-25)} \\ N_1 &= \frac{1}{\sqrt{1 + \frac{\beta}{\alpha(2 + \alpha z\tau_n + \beta z\tau_n)^2}}}; && \text{(A-26)} \\ N_2 &= -\frac{\alpha(\alpha + \beta)z^2 m^2}{2(\beta + \alpha^3 z^2\tau_n^2 + 2\alpha^2 z\tau_n(2 + \beta z\tau_n) + \alpha(2 + \beta z\tau_n)^2)}. && \text{(A-27)} \end{aligned}$$

The last step from Equations (A-24) to (A-25) involves Lemma 5. By taking the partial derivative of Equation (A-25) with respect to β and solving for $\frac{\partial E[U(W_n)]}{\partial \beta} > 0$. The solution consists of four regions: $\{\beta > \alpha, z < z_n^*\}$, $\{\beta > \alpha, z_n^* < z < z_n^{**}, m < m_n^*\}$, $\{\beta \leq \alpha, z < z_n^*\}$, and $\{\beta \leq \alpha, z > z_n^*, m < m_n^*\}$ where:

$$z_n^* = \frac{\sqrt{3}}{(\alpha + \beta)\tau_n}; \tag{A-28}$$

$$z_n^{**} = \frac{2}{(\beta - \alpha)\tau_n}; \tag{A-29}$$

$$m_n^* = \frac{(2 + \alpha z t_n - \beta z \tau_n)(4\alpha + \beta + \alpha z \tau_n(\alpha + \beta)(4 + z\tau_n(\alpha + \beta)))}{\alpha^2 z^2 (2 + \alpha z t_n + \beta z \tau_n)(\alpha^2 z^2 \tau_n^2 + 2\alpha\beta z^2 \tau_n^2 + \beta^2 z^2 \tau_n^2 - 3)}. \tag{A-30}$$

■

Proof of Corollary 3

Corollary 3 is proved by taking the limit ($z \rightarrow \infty$, $z \rightarrow 0$, $m \rightarrow 0$) of the conditions in Proposition 3. ■

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Market Reaction to the Adoption of IFRS in Europe

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ABSTRACT: This study examines European stock market reactions to 16 events associated with the adoption of International Financial Reporting Standards (IFRS) in Europe. European IFRS adoption represented a major milestone toward financial reporting convergence yet spurred controversy reaching the highest levels of government. We find an incrementally positive reaction for firms with lower quality pre-adoption information, which is more pronounced for banks, and with higher pre-adoption information asymmetry, consistent with investors expecting net information quality benefits from IFRS adoption. We find an incrementally negative reaction for firms domiciled in code law countries, consistent with investors' concerns over enforcement of IFRS in those countries. Finally, we find a positive reaction to IFRS adoption events for firms with high-quality pre-adoption information, consistent with investors expecting net convergence benefits from IFRS adoption.

Keywords: *IFRS; IAS 39; convergence; Europe.*

Data Availability: *All data are publicly available from sources indicated in the text.*

JEL Classifications: *M41, G15, G38.*

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I. INTRODUCTION

This study examines European stock market reactions to events associated with the 2005 adoption of International Financial Reporting Standards (IFRS) in Europe.[1] Prior to 2005, most European firms applied domestic accounting standards. Thus, the adoption of IFRS in Europe represented one of the largest financial reporting changes in recent years and was controversial, generating debate that reached the highest levels of government. The adoption of IFRS as issued by the International Accounting Standards Board (IASB) would result in the application of a common set of financial reporting standards within Europe, and between Europe and the many other countries that require or permit application of IFRS. Thus, the debate was about not only the benefits and costs of IFRS adoption itself, but also the global financial reporting convergence implications if IFRS were modified as a result of the adoption process.[2] Modifying IFRS would result in European standards differing from those used in other countries, thereby eliminating some potential convergence benefits.[3] We refer to the adoption of IFRS as issued by the IASB as the adoption of IFRS—adoption of modified standards is not adoption of IFRS. It is unclear how investors in European firms would react to this anticipated change in financial reporting. This study examines these reactions.

It is possible that investors in European firms would react positively to movement toward IFRS adoption if, for example, investors expected application of IFRS to result in higher quality financial reporting information, thereby lowering information asymmetry between the firm and investors and information risk and, thus, cost of capital. Investors also might have believed that application of a common set of standards would have convergence benefits, such as lowering the costs of comparing firms' financial position and performance across countries, and that IFRS adoption would enable European capital markets to become more globally competitive, with consequent increases in liquidity for European firms. Alternatively, it is possible that investors in European firms would react negatively to movement toward IFRS adoption. This could be the case if investors believed that IFRS would result in lower quality financial reporting information. For example, IFRS might not adequately reflect regional differences in economies that led to differences in domestic accounting standards. Also, investors might have believed that potential variation in the implementation and enforcement of IFRS would lead to an increase in the exercise of opportunistic managerial discretion when applying IFRS. Finally, investors might have believed that the implementation and transition costs associated with IFRS would exceed any benefits.

To gain insight into investors' expectations regarding the net cost or benefit of IFRS adoption in Europe, we examine three-day market-adjusted returns for firms with equity traded in the European stock market centered on 16 events that we assess as affecting the likelihood of IFRS adoption in Europe. We find an incrementally positive reaction for firms with lower pre-adoption information quality, which is consistent with investors expecting that IFRS adoption will result in greater informational benefits for these firms. We find an even more positive reaction for banks—but only those banks with lower pre-adoption information quality. This is consistent with investors expecting that IFRS will result in a greater improvement in information quality for these banks, perhaps reflecting perceived net benefits associated with adoption of the controversial International Accounting Standard (IAS) 39, *Financial Instruments: Recognition and Measurement.* We also find an incrementally positive reaction for firms with higher pre-adoption information asymmetry, which is consistent with investors expecting that IFRS adoption will mitigate this asymmetry. Finally, we find an incrementally negative reaction for firms domiciled in code law countries. This finding is consistent with investors harboring concerns regarding implementation of IFRS in countries that are generally thought to have weaker accounting standards enforcement, although this reaction could be attributable to other factors associated with a firm being domiciled in a code law country.

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[1] We examine market reactions in all stock markets in Europe. Throughout, we refer to these markets collectively as the European stock market. Also, we examine market reactions for firms trading in the European stock market. Throughout, we refer to these firms as European firms.

[2] For example, the U.S. Securities and Exchange Commission (SEC) recently eliminated the requirement for cross-listed firms that prepare their financial statements using IFRS as issued by the IASB to reconcile net income and shareholders' equity from those based on IFRS to those based on U.S. standards. The SEC did not propose to eliminate the requirement for cross-listed firms that use IFRS as modified by any particular jurisdiction, including the European Union.

[3] Market reactions studied in this paper may reflect perceptions of investors in European firms regarding the global convergence implications of any decision by Europe to modify IFRS before adoption.

Further analyses reveal a positive reaction to IFRS adoption events for the subset of European firms with the highest quality pre-adoption information. To the extent investors expect little, if any, informational benefits from IFRS adoption for these firms, this finding is consistent with investors expecting net benefits associated with convergence. Sensitivity analyses reveal that our results are robust to alternative proxies for information quality, measures of standards enforcement environments, and benchmark returns.

Section II discusses the background of IFRS adoption in Europe. Section III reviews prior research and provides the basis for interpreting the market reaction to each event. Section IV describes our data and research design. Section V presents our empirical results, and Section VI presents sensitivity analyses. Section VII concludes.

II. BACKGROUND

European Union Adoption Process

We focus on the European Union (EU) adoption of IFRS because the EU comprises most countries in Europe.[4] In March 2002, the European Parliament passed a resolution requiring all firms listed on stock exchanges of European member states to apply IFRS when preparing their financial statements for fiscal years beginning on or after January 1, 2005. This requirement affected approximately 7,000 firms.[5] The prospects of adopting IFRS represented a substantial shift in financial reporting for European firms because many requirements in IFRS differ from those in domestic standards of European countries. Also, the adoption of IFRS in Europe reflects an EU goal of achieving capital market integration; it is a necessary step toward convergence of financial reporting not only across Europe, but also between Europe and the rest of the world. Although the resolution requires firms to use IFRS, which are issued by the IASB, a private-sector standard-setter, the European Commission (EC) must endorse the standards before they are required in the EU. Thus, the EC retains the power to reject any standard, or part of a standard, it believes does not meet its criteria for endorsement. The three primary criteria are: the standard is not contrary to the EU's true and fair principle; the standard meets the criteria of understandability, relevance, reliability, and comparability; and adopting the standard is in the European public interest.

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[4] The exceptions are Iceland, Liechtenstein, and Norway, which are members of the European Economic Area (EEA), and Switzerland. Members of the EEA are committed to following EU Directives, including those relating to IFRS adoption. Switzerland is centrally located in Europe and, thus, has close economic ties with other European countries. Effective in 2005, Switzerland required all listed firms to use either IFRS or U.S. standards. Presumably its decision was related to that of the EU. Thus, our sample includes firms domiciled in Norway and Switzerland; no firms in Liechtenstein or Iceland met our data requirements. Because Switzerland is not committed to following EU Directives, we conducted all of our tests omitting Swiss firms with no change in our inferences.

[5] The required adoption date is January 1, 2007 for firms trading securities in the U.S. and basing their financial statements on U.S. standards, and firms trading debt securities but not equity securities.

The EC endorsement process, which played a key role in the adoption of IFRS in Europe, is as follows (Brackney and Witmer 2005; KPMG IFRG Limited 2005). The IASB develops IFRS in accordance with due process procedures outlined in its governing constitution (IASB 2006). This process involves public meetings and extensive input from interested parties around the world. Among these is the European Financial Reporting Advisory Group (EFRAG), a private-sector organization comprised of accounting experts from the EU, which provides advice to the EC regarding technical accounting matters. After the IASB issues a standard, EFRAG reviews it and, after public consultation, EFRAG decides whether to recommend that the EC endorse the standard for use in Europe. Taking EFRAG's advice into account, the EC drafts proposed regulation. The EC then seeks input from the Accounting Regulatory Committee (ARC). The ARC, a governmental organization comprised of representatives from each EU member state, reviews the regulation and provides its recommendation about adoption in the EU. The ARC considers the technical merits of the standard as expressed in EFRAG's recommendation letter, as well as the implications of the standard for the European public interest. If the ARC recommends endorsement, then the EC decides whether to endorse the standard, as written by the IASB or as amended, or to reject it. If endorsed, the standard becomes regulation applicable to firms in the EU. If the ARC recommends rejection of the standard, then the EC can ask EFRAG to consider it further, or send it to the European Parliament for a decision.[6]

The debate surrounding the adoption of IFRS in Europe initially focused on the merits of adopting IFRS, such as whether the benefits of the expected increase in capital flows would outweigh the costs of implementation and loss of diversity in domestic accounting standards. The debate later centered on IAS 39, *Financial Instruments: Recognition and Measurement*, and, to a lesser extent, on IAS 32, *Financial Instruments: Disclosure and Presentation* (IASB 2004a). The provisions in these two standards, particularly IAS 39 (IASB 2004b), had the potential to materially affect financial statement amounts for firms with a large number of financial instruments, notably banks. The debate regarding IAS 39 ultimately led to the modification of IAS 39 for adoption in Europe. However, modifications of IAS 39, or any other IASB standard, undermine the EU's goal of adopting a set of global standards.

Regarding IAS 39, the controversy focused on two types of requirements. The first type relates to use of fair value as the measurement attribute. IAS 39 requires many financial instruments—notably derivatives—to be recognized at fair value, with changes in fair value recognized in profit or loss.[7] IAS 39 also includes a fair value option that permits firms to designate irrevocably financial instruments on initial recognition as ones to be measured at

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[5] The process can apply to a single standard or to a group of standards. For the initial endorsement of IFRS in Europe, the extant set of standards was considered as a group. Specifically, the EC considered all standards effective at March 1, 2002, which included IAS 1 through IAS 41, as well as the related Standing Interpretations Committee (SIC) interpretations, i.e., SIC 1 through SIC 33.

[7] IAS 39 classifies financial assets into (1) loans and receivables not held for trading; (2) held-to-maturity investments; (3) financial instruments held for trading, including derivatives; and (4) available-for-sale financial assets. Financial assets in (1), (2), (3), and (4) are recognized at, respectively, amortized cost; amortized cost subject to impairment; fair value with changes in fair value recognized in profit or loss; and fair value with changes in fair value recognized in other comprehensive income. Most financial liabilities are recognized at amortized cost, except derivatives and liabilities held for trading, which are recognized at fair value with changes in fair value recognized in profit or loss.

fair value with changes in fair value recognized in profit or loss. The second type of controversial requirements relates to qualifying criteria for hedge accounting—IAS 39's qualifying criteria are specific and not easy to satisfy. Hedge accounting generally results in gains (losses) on a hedged item and losses (gains) on a designated hedging instrument being recognized in profit or loss at the same time to the extent the gains (losses) on the hedged item result from the hedged risk(s). Thus, hedge accounting reduces volatility in profit or loss resulting from, e.g., measuring derivatives entered into for hedging purposes at fair value and measuring the hedged item at amortized cost. However, IAS 39 does not permit hedge accounting for many financial instruments ostensibly entered into for hedging purposes. For example, IAS 39 does not permit hedge accounting for interest rate risk associated with core demand deposits, even though European banks frequently claim to hedge such risk. For many European firms, the fair value and hedging requirements in IAS 39 differ substantially from requirements in their domestic standards. In fact, most European domestic standards do not include standards specifying the financial reporting for many financial instruments.

IFRS Adoption Events

Although the adoption of IFRS was an involved process with considerable discussion, we identify 16 events between 2002 and 2005 that we assess as affecting the likelihood that IFRS would be adopted in Europe.[8] We identify the events by searching Dow Jones News Retrieval using the terms "IFRS," "international financial reporting standards," "harmonization," and "IAS 39," as well as by examining press releases and available listings of documents publicly released by the IASB, the European Parliament, the EC, and EFRAG. This search provided an initial listing of approximately 40 events. Each author independently verified each event's timing, content, and likely directional effect on IFRS adoption likelihood. Each author then independently identified the events that likely had the greatest effect on the likelihood of IFRS adoption; events that simply confirmed an earlier event were eliminated. Table 1 lists the resulting 16 events and our assessment as to whether each event increased or decreased adoption likelihood. We assess 13 events as increasing the likelihood of IFRS adoption and three events as decreasing it. Our assessment of each event's directional effect on adoption likelihood reflects our assessment of how the event likely affected investors' expectations conditional on prior events and discussions, and enables us to aggregate the market reaction across events.

The first event is March 12, 2002, when the European Parliament passed the resolution requiring all firms listed on stock exchanges in the EU to apply IFRS by 2005. The resolution passed by a vote of 429 for, 5 against, and 29 abstentions, indicating broad support for adoption of IFRS. Even though convergence toward international standards had been under consideration in Europe prior to 2002, we use the March 12, 2002 resolution as our first event because passage of the resolution was the first clear commitment to IFRS

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[8] Earlier events that we identify generally relate to broad adoption of IFRS in Europe. Later events generally focus on whether Europe might adopt particular standards as written by the IASB. Even though the focus was on particular standards for the later events, the uncertainty remaining to be resolved as the later events unfolded was more general. In particular, a key dimension of uncertainty was whether Europe would adopt IFRS as written by the IASB or would modify some standards, which could have global convergence implications.

TABLE 1
Events and Predicted Effects on Likelihood of European Adoption of IFRS

Event Date	Description	Assessed Effect on Likelihood of IFRS Adoption	Predicted Market Reaction if $\text{IFRS}_{\text{benefits}} > \text{IFRS}_{\text{costs}}$ $(\text{IFRS}_{\text{benefits}} < \text{IFRS}_{\text{costs}})$
March 12, 2002	European Parliament passes resolution requiring all EU-listed companies to use IFRS by 2005	Increase	+ (−)
May 14, 2002	EFRAG issues draft recommendation to endorse all extant IFRS	Increase	+ (−)
June 19, 2002	EFRAG issues final recommendation to endorse all extant IFRS	Increase	+ (−)
July 4, 2003	Chirac sends letter to Prodi expressing concerns about IAS 39 and its potential negative effect on Europe	Decrease	− (+)
July 9, 2003	Bolkestein sends letter to Tweedie supporting goal of adoption	Increase	+ (−)
July 16, 2003	ECOFIN and ARC support adoption of IFRS	Increase	+ (−)
September 29, 2003	EC endorses all extant IFRS, except IAS 32 and IAS 39	Increase	+ (−)
February 3, 2004	Bolkestein pledges to postpone endorsement of IAS 32 and IAS 39 until issues are resolved; sets up consultative group to facilitate resolution	Increase	+ (−)
March 30, 2004	HSBC announces intentions to implement IAS 39 in full	Increase	+ (−)
June 4, 2004	EFRAG issues draft recommendation to endorse IAS 32 and IAS 39	Increase	+ (−)
July 8, 2004	EFRAG issues final recommendation to endorse IAS 32 and IAS 39	Increase	+ (−)
October 1, 2004	ARC recommends endorsement of IAS 39, but recommends provisions relating to the fair value option and portfolio hedging of demand deposits be carved out	Decrease	− (+)
November 19, 2004	EC endorses IAS 39 with both carve-out provisions	Decrease	− (+)
June 16, 2005	IASB issues revised IAS 39 with new fair value option	Increase	+ (−)
July 8, 2005	ARC recommends endorsement of revised fair value option, thereby eliminating one of the carve-outs	Increase	+ (−)
November 15, 2005	EC endorses revised fair value option, thereby eliminating one of the carve-outs	Increase	+ (−)

(continued on next page)

TABLE 1 (continued)

This table presents the 16 events, our assessment of their effect on the likelihood of the European adoption of IFRS as issued by the IASB, and the predicted market reaction to each event. In the last column, $IFRS_{benefits} > IFRS_{costs}$ refers to the predicted market reaction if expected benefits associated with IFRS adoption exceed expected costs. $IFRS_{benefits} < IFRS_{costs}$ refers to the predicted market reaction if expected benefits associated with IFRS adoption are lower than expected costs.

Key persons/organizations referred to in the event descriptions are defined as follows:

- ARC (Accounting Regulatory Committee) is a public-sector body that opines on EC proposals regarding international accounting standards, and is comprised of representatives from each member state of the European Union (EU), chaired by the EC.
- Bolkestein (Frits Bolkestein), a commissioner of the EC, is responsible for internal markets, taxation, and customs union.
- Chirac (Jacques Chirac) is the President of France.
- EC (European Commission) was created to represent the European interest common to all Member States of the EU, and has the right of initiative in the legislative process, i.e., it proposes the legislation on which the European Parliament and the Council decide to enact.
- ECOFIN (European and Financial Affairs Council) is comprised of the Economics and Finance ministers of the member states, and covers EU policy in several areas, including financial markets.
- EFRAG (European Financial Reporting Advisory Group) is a private-sector body created by the accounting profession within Europe, and advises the EC on the technical assessment of IASB-issued financial reporting standards.
- IASB (International Accounting Standards Board) is an independent, privately funded financial reporting standard-setter charged with creating International Financial Reporting Standards.
- Prodi (Romano Prodi) is the President of the EC.
- Tweedie (Sir David Tweedie) is the Chairman of the IASB.

adoption.[9] On May 14, 2002 and June 19, 2002, EFRAG issued its draft and final recommendations that extant IFRS be endorsed *en bloc*. The endorsement recommendation letters stated EFRAG's view that the regulation requiring European firms to adopt IFRS by 2005 is a major achievement in that the common basis for financial reporting based on high-quality global standards provides a platform for efficient cross-border investment both within and beyond the EU. EFRAG further noted that the IASB was reviewing several standards with the objective of making some improvements to them; EFRAG would consider those changes and make its recommendation on them after the IASB issues them. These three events reflected clear support for the broad adoption of IFRS. Therefore, we assess these events as increasing the likelihood of IFRS adoption in Europe.

During the remainder of 2002 and into 2003 the EC considered whether to accept EFRAG's recommendation to endorse extant IFRS. On July 4, 2003, Jacques Chirac, President of France, wrote a letter to Romano Prodi, President of the EC, to express concern that adopting IFRS, particularly IAS 39, would not be in the best interest of Europe. Chirac's interest in the debate arose at least in part because French banks were among the most critical of IAS 39. Chirac's involvement showed IFRS-related concern at the highest level of government. Therefore, we assess this event as decreasing the likelihood of IFRS adoption.

On July 9, 2003, Frits Bolkestein, the EC commissioner responsible for internal markets, expressed to Sir David Tweedie, Chairman of the IASB, similar concern about IAS 39, but expressed support for the broader goal of convergence using IFRS. On July 16, 2003, the ARC and the EU's Economic and Financial Affairs Council (ECOFIN), which is comprised of the Economics and Finance ministers of the EU member states, echoed Bolkestein's support for adoption of IFRS, despite also echoing his concern about IAS 39. On September 29, 2003, the EC endorsed all extant IFRS, except IAS 32 and IAS 39. Even though all of these events reflect concern about IAS 39, they all expressed clear support for IFRS adoption and the desire to work to resolve in the near term the remaining issues relating to IAS 39. Therefore, we assess these three events as increasing the likelihood of IFRS adoption.

After the EC's endorsement of most extant IFRS, the debate seemingly focused on IAS 32 and IAS 39, although the debate also reflected the possibility that the EU would amend IASB-issued standards—and the implications this might have for the adoption of future standards by the EU. Although the IASB revised IAS 39 in December 2003, the revisions did not resolve the controversial issues relating to the fair value option and hedge accounting requirements. Thus, the endorsement of IAS 32 and IAS 39 remained uncertain.

On February 3, 2004, Bolkestein indicated his intention to continue postponing endorsement of IAS 32 and IAS 39 until the issues could be resolved. To facilitate resolution, he announced establishment of a high-level consultative group. On March 30, 2004, HSBC, the largest European bank, expressed its support for adoption by announcing its plans to implement IAS 39 in full, regardless of whether the EC endorsed it. On June 4, 2004, EFRAG issued draft recommendations to endorse IAS 32 and IAS 39. Although the IAS 32 recommendation was unanimous, 6 of 11 EFRAG members voted against the IAS 39 recommendation. A majority negative vote is insufficient for EFRAG to recommend non-endorsement of a standard—the EFRAG constitution requires a two-thirds negative vote. On July 8, 2004, EFRAG issued its final recommendations to endorse IAS 32 and IAS 39, both based on the same votes as the draft recommendations. Although each of these events continued to reflect concern regarding IAS 39, the actions taken by Bolkestein to resolve the conflicts, the support of IAS 39 by a major European bank, and EFRAG's recommended endorsement of IAS 39 all reflected events that we assess as having increased the likelihood of IFRS adoption.

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[9] Prior research finds little (Comprix et al. 2003) or limited (Pae et al. 2007) evidence of a market reaction to IFRS adoption events before March 12, 2002. These findings suggest that investors' IFRS adoption expectations were not affected by events prior to the March 12, 2002 resolution, the studied events were not those that affected investors' expectations, or the event windows were too narrow to capture the market reaction to the events. Nonetheless, our findings may only partially capture the market reaction to EU IFRS adoption to the extent that events prior to March 12, 2002 affected investors' expectations about the European Parliament's resolution. The direction of any bias arising from the omission of such events is unclear.

On October 1, 2004, the ARC added its endorsement recommendation to that of EFRAG. However, the ARC did not recommend endorsement of IAS 39 as issued by the IASB. Rather, the ARC recommended that the EC "carve out" of IAS 39 the two parts of the standard that were the focus of the controversy. Endorsing IAS 39 with this carve-out would mean that IFRS as applied in Europe would differ from IFRS applied elsewhere in the world, thereby thwarting the goal of global convergence described in the 2002 EFRAG endorsement recommendation letters. On November 19, 2004, the EC followed the ARC recommendation and endorsed a carve-out version of IAS 39. Because these two events indicated that the EC was willing to alter IFRS, we assess these events as decreasing the likelihood of IFRS adoption.

The EC indicated its intention that the carve-outs be temporary, only in place until the technical controversies were resolved. On June 16, 2005, the IASB revised the fair value option in IAS 39, and on July 8, 2005 ARC recommended endorsement of it. The EC endorsed the revised fair value option on November 15, 2005, thereby eliminating one of the two carve-outs of IAS 39.[10] Because these three events supported the EC's intention to eliminate the carve-outs and made IFRS as endorsed in the EU closer to IFRS as issued by the IASB, we assess these events as increasing the likelihood of IFRS adoption.

III. PRIOR RESEARCH AND EXPECTED MARKET REACTION

Little is known about how investors perceived the possibility of IFRS adoption in Europe. This study infers investor perceptions by examining the equity market reaction to events leading to the adoption. Prior research uses this approach to assess the perceptions of investors in U.S. firms regarding individual standards (e.g., fair value accounting in Statement of Financial Accounting Standards [SFAS] No. 115 by Beatty et al. [1996] and Cornett et al. [1996]; and stock-based compensation accounting in SFAS No. 123 by Dechow et al. [1996]). However, the setting in our study, which investigates investor perceptions regarding an entire set of accounting standards, is perhaps more analogous to prior research that examines investor perceptions to broad legislation (e.g., the Sarbanes-Oxley Act by Jain and Rezaee [2006], Zhang [2007], and Li et al. [2008]).

It is unclear how investors in European firms would react to movement toward IFRS adoption. It is possible that investors would react positively to movement toward IFRS adoption if, for example, they expected application of IFRS to result in higher quality financial reporting relative to application of domestic standards, thereby enhancing financial reporting transparency, and reducing information asymmetry and information risk and, thus, lowering cost of capital. This prediction is supported by prior research. For example, Barth et al. (2008) finds that application of International Accounting Standards (IAS), which comprise a large portion of extant IFRS, is associated with higher quality accounting amounts than application of non-U.S. domestic standards. Similarly, Karamanou and

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[10] As of the writing of this manuscript, the second carve-out relating to hedge accounting remains in place.

Nishiotis (2005) finds positive abnormal returns for a small set of non-U.S. firms announcing voluntary adoption of IAS between 1989 and 1999. Diamond and Verrecchia (1991), Baiman and Verrecchia (1996), Leuz and Verrecchia (2000), and Barth et al. (2009), among others, find that higher financial reporting quality is associated with lower cost of capital. These studies are consistent with Aboody et al. (2004) and Easley and O'Hara (2004), which provide evidence that information risk is priced and, thus, its perceived reduction could result in an empirically detectable market reaction.

Investors might also react positively to movement toward IFRS adoption if they expect application of IFRS to have positive cash flow effects. These effects could include reduced contracting costs (e.g., Beatty et al. 1996) or reduced scope for managerial rent extraction associated with greater financial reporting transparency (e.g., Hope et al. 2006). It also is possible that investors in European firms would react positively to movement toward IFRS if they believed IFRS would provide convergence benefits. For example, Barth et al. (1999) finds that there can be positive market effects associated with convergence.[11] Similarly, Ashbaugh and Pincus (2001) finds that previous convergence efforts relating to IAS resulted in reductions in analyst forecasts errors. Pae et al. (2007) finds that firm value reflected in Tobin's Q, increased after the passage of EU regulations intended to converge financial reporting, particularly for firms with higher agency costs.

Alternatively, it is possible that investors in European firms would react negatively to movement toward IFRS if, for example, they believed IFRS would decrease financial reporting quality. This could occur if investors believed IFRS would fail to either adequately reflect regional differences in economies or accommodate countries' differing political and economic features that led to existing differences in domestic accounting standards.[12] Investors might also believe that variation in the implementation and enforcement of IFRS could lead to an increase in opportunistic managerial discretion when applying IFRS. Ball (1995, 2006) and Daske et al. (2007), among others, point out that effective financial reporting convergence requires consistent implementation and enforcement of standards. Unlike the SEC in the U.S., there was no regulatory counterpart with enforcement authority that spanned the European member states to ensure consistent application of IFRS. Consistent with factors other than standards themselves affecting financial reporting quality, Ball et al. (2003) reports no detectable information quality difference between East Asian firms with high-quality, i.e., common law-based, accounting standards and those with low-quality, i.e., code law-based, accounting standards. There also is evidence that substantial information quality differences within Europe remain even after convergence efforts that preceded the 2005 IFRS adoption EU mandate (e.g., Tay and Parker 1990; Joos and Lang 1994). Investors also might have believed that any convergence benefits arising from adoption of IFRS would be less than the costs to implement and transition to the new set of standards.

IV. DATA AND RESEARCH DESIGN

We infer investor perceptions relating to IFRS adoption by examining European firms' equity return reactions to our 16 adoption events. We first provide descriptive evidence on the overall European market reaction to these events. We then focus our tests on determining whether particular firm characteristics explain cross-sectional variation in firms' return reactions in a manner consistent with our predictions. Because the adoption of IFRS resulted from a process that evolved over several years, we draw our inferences from analyses of the market reactions associated with all 16 events taken together, rather than with each event separately.

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[11] Barth et al. (1999) shows that the net market effect of convergence is a function of two effects. The first is the direct informational effect, i.e., whether convergence increases or decreases accounting quality. The second is the expertise acquisition effect, i.e., whether investors become experts in foreign accounting, which depends on how costly it is to develop the expertise. Therefore, *ex ante* the net market effect of convergence is uncertain.

[12] For example, relative to domestic standards, IFRS generally specify a greater use of fair values, which some believe is more susceptible to opportunistic managerial discretion than are modified historical cost-based amounts.

Our initial sample comprises all European firms for which event returns are available for all 16 events in Table 1, which yields an initial sample of 3,265 firms. Our cross-sectional tests require additional data, which results in a smaller sample size of 1,956 firms. We base our tests on three-day value-weighted market-adjusted returns for these firms centered on each of the 16 event dates, $CMAR_{j,e}$, where j denotes firm and e denotes event.[13] We obtain daily price data between 2002 and 2005 from Datastream through Thomson One Banker Analytic. Table 2 provides a breakdown of the sample by country.

TABLE 2
Sample Composition by Country

Country	Firms	Total Obs.
Austria	39	624
Belgium	65	1,040
Czech Republic	5	80
Denmark	80	1,280
Finland	84	1,344
France	424	6,784
Germany	518	8,288
Greece	150	2,400
Ireland	32	512
Italy	202	3,232
The Netherlands	109	1,744
Norway	74	1,184
Poland	45	720
Portugal	38	608
Spain	94	1,504
Sweden	187	2,992
Switzerland	149	2,384
U.K.	970	15,520
Total	3,265	52,240

This table presents the sample composition by country. The sample includes all European firms with returns available for all 16 events in 2002–2005 listed in Table 1.

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[13] We obtain the same inferences if we require sample firms to have a three-day return for only one event. In addition, untabulated statistics provide additional assurance that our sample firms are representative of European firms. In particular, the Pearson correlations between daily non-event date returns for the initial sample portfolio and daily non-event returns for the MSCI European Index, which is a broad-based market index, and the 600 European firms included in the DJ STOXX Global 1800 Index are 0.993 and 0.985, respectively. The correlation between the portfolio returns on event and non-event dates for our initial sample and the sample on which we base our cross-sectional analyses is 0.995.

Following the vast event-study literature (e.g., Campbell et al. 1997), we market-adjust raw event returns to mitigate potentially confounding effects of global news occurring concurrently with our event dates. Because our events affect a large subset of the global equity market—all of Europe—the appropriate market index is not obvious. We market adjust the returns by subtracting the corresponding three-day return to the Dow Jones STOXX Global 1800 Index excluding the 600 European firms in the index (DJ STOXX 1800 ex Europe, hereafter).[14] We base our inferences on the DJ STOXX 1800 ex Europe index because including returns of European firms in the market adjustment return would remove some of the effect we seek to document. However, untabulated findings reveal that our inferences are unchanged if we use the full DJ STOXX 1800 Index. Consistent with the existence of global news that affects returns of European firms and firms in other parts of the world, untabulated statistics reveal a significantly positive Pearson correlation of 0.551 between daily non-event returns for our initial sample of European firms and daily non-event returns for the DJ STOXX 1800 ex Europe.[15,16]

Our event-study research design relies on a degree of equity market efficiency in the sample countries that is sufficient to ensure the information related to each event is reflected in equity prices during the event window in an unbiased manner. In particular, the maintained hypothesis throughout our analysis is that equity prices reflect unbiased expectations of the costs and benefits of IFRS adoption conditional on available information. Although the size and liquidity of the European equity market suggests this is a tenable assumption, there is likely variation across markets within Europe during our sample period. If a sample country's equity market is not sufficiently efficient to reflect event information within the event window, our tests can lack power or be biased (e.g., Hirshleifer 2001).

Our tests also rely on both the correct identification of information events and there being no confounding news during the event windows.[17] Including non-events likely introduces noise and excluding relevant events likely reduces power; both of which can introduce bias. Our event selection procedures, described in Section II, are intended to minimize the likelihood of including non-events and excluding relevant events.[18] Regarding potentially confounding news, we search the U.S. and European editions of the *Wall Street Journal*, including "World Markets" articles, and the European edition of the *Financial Times* for non-IFRS related news during each event window. We observe other news within nearly all event windows. However, as Table 1 indicates, our predictions are based on one of two patterns of market reactions across event dates, depending on whether investors perceive that the benefits of IFRS adoption exceed the costs, or *vice versa.* Our search for confounding news revealed no discernible pattern of good (bad) news during event windows for events that we assess as increasing (decreasing) the likelihood of IFRS adoption, or *vice versa.* That is, we do not observe a systematic alternative news pattern that would bias our inferences.

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[14] The stated objective of the Dow Jones STOXX Global 1800 Index (hereafter, DJ STOXX 1800) is to "provide a broad yet investable representation of the world's developed markets." The index comprises the largest 600 firms, based on free float market capitalization, from each of Europe, North and South America, and the Asia/Pacific region.

[15] Our sample comprises all European firms that meet our data requirements, including, but not limited to, the 600 largest European firms that are in the DJ STOXX 1800 Index. We do not limit our analyses to the European firms in the index for three reasons. First, IFRS adoption would affect all firms in the European market, not only the largest firms. Thus, limiting our analyses to the largest firms limits the generalizability of our inferences. Second, we predict and find evidence that investors' reaction to European IFRS adoption events is smaller for larger firms. Thus, limiting our analyses to the largest firms could bias against us finding a significant market reaction across our event dates. Third, limiting the analyses to the largest firms would reduce cross-sectional variation in our sample on dimensions relevant to our cross-sectional predictions, thereby reducing the power of our tests.

[16] It is possible that on our event dates investors in non-European firms revised their expectations of IFRS adoption outside of Europe, which would affect returns that comprise the DJ STOXX 1800 ex Europe index. If so, then using the DJ STOXX 1800 ex Europe index to market-adjust our event returns could remove some of the effect we seek to document, i.e., bias our market-adjusted returns toward zero. Nonetheless, we use the index to market adjust our returns because, as for European firms, we expect that any IFRS adoption effect is relatively small for the largest non-European firms, whose returns comprise the index, which mitigates against such bias.

[17] See Campbell et al. (1997, Chapter 4) for a detailed discussion of the assumptions and limitations of an event-study research design.

Overall European Market Reaction

Our descriptive evidence on the overall European market reaction to IFRS adoption in Europe focuses on market-adjusted event returns for a value-weighted portfolio of the initial sample of firms. We view this evidence as descriptive because of the challenge in identifying the appropriate market index with which to adjust the returns. We construct portfolio event returns by value weighting each firm's return based on the firm's equity market value at the end of the most recent quarter prior to the event to obtain a portfolio $CMAR_e$ for each event. We base our test statistics on portfolio returns because each portfolio return is unaffected by potential cross-sectional correlation (Sefcik and Thompson 1986), and portfolio returns for different events should be uncorrelated. Because we assess some events as increasing and some as decreasing the likelihood of IFRS adoption, when calculating our test statistics we multiply the event returns for the latter by -1 to enhance interpretation of investors' response to the aggregated events.

We provide three statistics to test the significance of the portfolio event returns. The first is a t-test of whether the mean of the 16 event portfolio $CMAR_e$s differs from zero. This statistic assumes normality of the return distribution and that our market adjustment is the proper benchmark for the expected market return and, thus, the expected market-adjusted return equals zero. The standard deviation used to calculate the statistic is derived from the distribution of the 16 event portfolio returns. Consistent with Fama and MacBeth (1973), this assumes portfolio returns associated with different events are uncorrelated.

The second statistic is a t-test for whether the mean of the 16 event portfolio $CMAR_e$s differs from the mean of a distribution of similarly constructed non-event portfolio returns. This statistic assumes unequal variances for the event and non-event return distributions. We present this statistic because it admits the possibility that returns of European firms differ systematically from returns of firms in other regions. That is, the statistic does not assume that our market adjustment fully adjusts for the market return. To the extent returns of European firms do not differ systematically from other firms, this statistic will display diminished power because of estimation noise. To form the distribution of non-event returns, we use returns for European firms for all days in our event years, 2002 to 2005, that do not overlap with our event windows. We calculate the 300 three-day portfolio returns, ensuring non-overlapping windows, and subtract the corresponding three-day return to the DJ STOXX 1800 ex Europe Index.[19]

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[18] It is possible that the June 19, 2002 and July 8, 2004 events, which finalized EFRAG endorsements, only confirmed investors' prior expectations and did not provide new information. This possibility requires assuming EFRAG's final endorsement is perfunctory. If this is the case, they would be non-events and, thus, including them in our tests likely would add noise. Regardless, untabulated findings reveal that our inferences are unaffected by excluding firms' returns associated with these two events. Also, some might interpret the February 3, 2004 event as signaling a decreased likelihood of IFRS adoption because Bolkestein delayed endorsement of two standards. Untabulated findings also reveal that our inferences are unchanged if we adopt this alternative interpretation of this event.

The third statistic reflects the probability that the mean of the 16 event portfolio $CMAR_e$s exceeds the mean of 16 similarly constructed randomly selected non-event portfolio returns. This statistic assumes the distribution of these non-event returns is the same as the distribution of event returns, but does not rely on any other distributional assumptions. However, to the extent the randomly selected non-event portfolio returns do not reflect the population of portfolio returns, there could be noise or bias in this statistic. To construct this statistic, we randomly select 16 non-event portfolio returns from non-event dates that mimic the year-by-year distribution of our sample events. That is, we select three, four, six, and three non-event portfolio returns from 2002, 2003, 2004, and 2005, respectively. We then designate one of the non-event portfolio returns from 2003 and two from 2004 as being associated with events that decrease the likelihood of IFRS adoption. We then compare the standardized mean of the non-event portfolio returns to the standardized mean of the 16 event portfolio $CMAR_e$s.[20] We repeat this procedure 500 times to construct a simulated p-value for the probability that the standardized mean non-event portfolio return is greater than the standardized mean portfolio $CMAR_e$.[21]

Cross-Sectional Analysis

We base our main inferences on tests of whether firm characteristics explain cross-sectional variation in the market reaction to IFRS adoption events. This analysis assumes that investors assess the expected costs and benefits of IFRS adoption, including those related to accounting information quality, and implementation and transition. To obtain our inferences, we estimate the following equation:

$$\begin{aligned} CMAR_{j,e} = {} & \beta_0 + \beta_1 InfoQualFactor_{j,e} + \beta_2 Bank_{j,e} \\ & + \beta_3 InfoQualFactor_{j,e} * Bank_{j,e} + \beta_4 Turnover_{j,e} \\ & + \beta_5 CloselyHeld_{j,e} + \beta_6 Herf_{j,e} + \beta_7 Code_{j,e} + \beta_8 Big4_{j,e} + \varepsilon_{j,e}. \end{aligned} \tag{1}$$

As in our portfolio return statistics, when estimating Equation (1) we multiply by −1 returns associated with events we assess as decreasing the likelihood of IFRS adoption.

Our proxy for the firm's pre-adoption information quality is *InfoQualFactor*, which is the first principal component derived from four variables selected to capture information quality.[22] The four variables are *ADR*, an indicator variable that equals 1 if a firm cross-lists in the U.S. using American Depository Receipts (ADR) during the event year, and 0 otherwise; *Standards Applied*, an indicator variable that equals 1 if the firm applies U.S. standards or International Accounting Standards (IAS) during the event year, and 0 if the firm applied domestic standards;[23] *Exchanges*, the number of exchanges on which the firm is listed during the event year; and *Size*, the natural logarithm of the firm's prior end of year market value of equity. We expect ADR firms to have higher quality pre-adoption information because these firms are subject to U.S. securities regulation and enforcement, are required during our sample period to reconcile domestic standards-based net income and equity book value to those based on U.S. standards, and are typically larger and more widely followed by analysts.[24] We expect that firms applying U.S. standards or IAS, firms listed on multiple exchanges, and firms that have larger equity market values to have higher quality pre-adoption information. To ease interpretation, we multiply factor scores by -1 so that higher values of *InfoQualFactor* correspond to lower quality information. If investors perceive the benefits to IFRS adoption are higher for firms with lower quality pre-adoption information, then we expect β_1 is positive.

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[19] There are approximately 75 non-event three-day windows in each of the four sample years, which results in 300 non-event return windows. To ensure that our non-event returns do not overlap with each other, we select every fourth trading day as the beginning of the three-day return window. Our inferences are unaffected by altering the starting point for this trading day selection.

[20] We standardize means by dividing the mean return by the standard error of the 16 event return distribution.

[21] This procedure effectively calculates how often our event distribution t-statistic is larger than a t-statistic estimated from a similarly constructed distribution of non-event dates. Bootstrap p-values indicate the likelihood of obtaining a similar magnitude statistical rejection of the null hypothesis on non-event dates. See, for example, Hein and Westfall (2004).

[22] *InfoQualFactor* is estimated using varimax orthogonal rotation. The first and second principal component eigenvalues are 1.75 and 0.99, respectively.

To provide insight into whether the event market reactions reflect perceptions regarding IFRS generally or IAS 39 in particular, we include in Equation (1) *Bank*, which is an indicator variable equal to 1 if the firm's primary two-digit SIC code is 60 or 61, i.e., depository institutions and non-depository credit institutions, and 0 otherwise. We include *Bank* because IAS 39 figured prominently in banks' resistance to EU IFRS adoption. Investors in banks might react more positively than investors in other firms if they perceive informational benefits associated with IAS 39, such as those associated with financial statement recognition of previously unrecognized derivative financial instruments, leading to a positive predicted sign for β_2. However, investors in banks might react more negatively if they perceive that banks would incur more IFRS-related transition costs, such as those associated with extensive systems changes to account for large portfolios of financial instruments and hedging activities or raising the attention of banking regulators if they report more volatile earnings based on IFRS than based on their domestic standards. This would lead to a negative predicted sign for β_2. Thus, we do not predict the sign of β_2. Equation (1) also includes the interaction variable *InfoQualFactor* * *Bank*, which is intended to capture any incremental market reaction for those banks with lower quality pre-adoption information as reflected in *InfoQualFactor*. For reasons discussed above, we do not predict the sign of β_3.

Equation (1) also includes three proxies for pre-adoption information asymmetry among investors or between the firm and investors. The first is *Turnover*, which is an indicator variable that equals 1 if the firm's ratio of average number of daily shares traded to average total number of shares outstanding for the year is greater than the sample median, and 0 otherwise. The second is *CloselyHeld*, which is the percentage of shares held by insiders, as provided by Worldscope. The third is *Herf*, which is the Herfindahl index, measured as the sum of squared market shares, i.e., percentage of total industry sales, for all firms in the firm's primary two-digit SIC code. Thus, *Herf* ranges from 0 to 1, with higher values indicating that within-industry sales are concentrated among fewer firms. We expect that firms with lower turnover, with greater insider ownership, and that have less industry competition have more information asymmetry. If investors expect IFRS adoption to reduce information asymmetry, then they will react more positively to increases in the likelihood of IFRS adoption for firms with greater pre-adoption information asymmetry. This would be consistent with investors perceiving a reduction in the firm's future cost of capital. Thus, we expect β_4 is negative, and β_5 and β_6 are positive. However, if investors perceive that IFRS adoption will require firms with dominant industry market share to disclose proprietary information, then we expect β_6 is negative.

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[23] Belgium, the Czech Republic, Denmark, Finland, Germany, the Netherlands, and Switzerland permitted firms a choice of accounting standards prior to mandatory IFRS adoption.

[24] We identify ADR firm years from the Bank of New York Complete Depository Receipt record (http://160.254.123.37/dr_directory.jsp), which indicates both the type and date of the ADR listing. We use only Level II or Level III ADRs, not Level I. This is because during our sample period firms with Level II and Level III ADRs were required to provide domestic-to-U.S. standards-based reconciliations and were subject to more stringent requirements than firms with Level I ADRs.

Finally, Equation (1) includes two proxies for enforcement and implementation of accounting standards. The first is *Code*, which is an indicator variable that equals 1 if a firm is domiciled in a code law country, and 0 otherwise.[25] Investors may expect that financial reporting standards are less stringently enforced in code law countries (Ball et al. 2000, 2003). Therefore, firms in code law countries may retain greater flexibility in the application of IFRS. If this is the case, we expect β_7 is negative. The second proxy is *Big4*, which is an indicator variable that equals 1 if the firm's auditor during the fiscal year is one of the four largest, as reported by Worldscope, and 0 otherwise. Prior research finds that larger audit firms provide higher quality audits (DeAngelo 1981), and that investors perceive and price quality differences associated with stronger monitors (e.g., Hogan 1997; Muller and Riedl 2002). Thus, investors may expect larger audit firms to provide more stringent enforcement and have more resources available to facilitate IFRS transition. If this is the case, we expect β_8 is positive.

To account for potential cross-sectional correlation among the residuals from Equation (1), we calculate two-way clustered standard errors based on two-digit industry and country (Rogers 1993; Gow et al. 2010; Petersen 2009).[26] We cluster along these two dimensions because it is reasonable to assume that financial reporting practices and changes therein are more homogeneous within industries and countries than across industries and countries.

V. RESULTS

Overall European Market Reaction

Table 3 presents the portfolio event returns statistics. For each of our 16 events, we present the raw return to the portfolio of 3,265 European firms (column labeled "Raw Return Europe"); the DJ STOXX 1800 ex Europe index (column labeled "DJ STOXX 1800 ex Europe Index Return"); and the difference between them, which is the market-adjusted European return (column labeled "Market-Adjusted Return Europe"). Table 3 presents the individual event portfolio returns with their predicted and actual signs; none of the individual event portfolio returns is significantly different from zero.

To compute the mean of the 16 event returns, which we present at the bottom of the table, we multiply by -1 returns associated with events we assess as decreasing the likelihood of IFRS adoption. Thus, we predict that the mean of the event portfolio returns is positive if the benefits to IFRS adoption outweigh the costs. We find that the mean raw return associated with the 16 events for the European portfolio is -0.0052, versus -0.0086 for the DJ STOXX 1800 ex Europe index. The difference of 0.0034 is positive and significantly different from zero ($\text{t-statistic}_{vs0} = 2.627$) and significantly different from the mean difference for the non-event returns ($\text{t-statistic}_{vs300} = 1.980$). Further, bootstrap estimation reveals there is less than a 1 percent chance of randomly drawing 16 non-event returns with a standardized mean larger than the mean for our 16 events ($\text{p-value}_{bootstrap} = 0.008$).[27]

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[25] All of our sample countries except the U.K. and Ireland are classified as code law countries.

[26] All of our inferences are unaffected by clustering on either two-digit industry or country.

[27] Our inferences are the same when we use equal-weighted portfolio returns. Our inferences also are the same if we market adjust the portfolio returns by subtracting $\hat{\alpha} + \hat{\beta} * \text{DJ STOXX 1800 Index ex Europe}$, where $\hat{\alpha}$, $\hat{\beta}$ are coefficient estimates from a regression of raw portfolio returns on the index returns for non-event days during the year preceding each event (Brown and Warner 1980, 1985).

TABLE 3
Overall European Market Reaction to Events Affecting the Likelihood of IFRS Adoption in Europe

Event Date	Description	Predicted Sign	Raw Return Europe	DJ STOXX 1800 ex Europe Index Return	Market-Adjusted Return Europe
March 12, 2002	European Parliament passes resolution requiring all EU-listed companies to use IFRS by 2005	+	−0.0116	−0.0136	0.0020
May 14, 2002	EFRAG issues draft recommendation to endorse all extant IFRS	+	0.0199	0.0307	−0.0108
June 19, 2002	EFRAG issues final recommendation to endorse all extant IFRS	+	−0.0403	−0.0441	0.0038
July 4, 2003	Chirac sends letter to Prodi expressing concerns about IAS 39 and its potential negative effect on Europe	−	0.0249	0.0317	−0.0068
July 9, 2003	Bolkestein sends letter to Tweedie supporting adoption	+	−0.0110	−0.0163	0.0053
July 16, 2003	ECOFIN and ARC support adoption of IFRS	+	−0.0132	−0.0163	0.0031
September 29, 2003	EC endorses all extant IFRS, except IAS 32 and IAS 39	+	−0.0210	−0.0191	−0.0019
February 3, 2004	Bolkestein pledges to postpone endorsement of IAS 32 and IAS 39 until issues are resolved; sets up consultative group to facilitate resolution	+	−0.0026	−0.0123	0.0097
March 30, 2004	HSBC announces intentions to implement IAS 39 in full	+	0.0090	0.0012	0.0078
June 4, 2004	EFRAG issues draft recommendation to endorse IAS 32 and IAS 39	+	0.0181	0.0112	0.0069
July 8, 2004	EFRAG issues final recommendation to endorse IAS 32 and IAS 39	+	−0.0006	−0.0098	0.0092

(continued on next page)

TABLE 3 (continued)

Event Date	Description	Predicted Sign	Raw Return Europe	DJ STOXX 1800 ex Europe Index Return	Market-Adjusted Return Europe
October 1, 2004	ARC recommends endorsement of IAS 39, but recommends provisions relating to the fair value option and portfolio hedging of demand deposits be carved out	−	0.0213	0.0272	−0.0059
November 19, 2004	EC endorses IAS 39 with both carve-out provisions	−	−0.0096	−0.0063	−0.0033
June 16, 2005	IASB issues revised IAS 39 with new fair value option	+	0.0048	−0.0008	0.0056
July 8, 2005	ARC recommends endorsement of revised fair value option, thereby eliminating one of the carve-outs	+	0.0067	0.0050	0.0017
November 15, 2005	EC endorses revised fair value option, thereby eliminating one of the carve-outs	+	−0.0040	−0.0005	−0.0035
	Mean Return across Events	+	−0.0052	−0.0086	0.0034
	t-statistic$_{vs0}$				2.627
	t-statistic$_{vs300}$				1.980
	p-value$_{bootstrap}$				0.008

This table presents three-day portfolio returns centered on the 16 events identified as affecting the likelihood of IFRS adoption in Europe. Raw Return Europe is the three-day value-weighted return to the 3,265 European firm portfolio, centered on the event date. DJ STOXX ex Europe Index Return is the three-day value-weighted return to the DJ STOXX 1800 ex Europe Index, centered on the event date. Market-Adjusted Return Europe is the difference between Raw Return Europe and DJ STOXX 1800 ex Europe Index Return. Predicted sign relates to predictions for the sign of Market-Adjusted Return Europe. Mean Return across Events is computed as the mean of the individual event returns, after multiplying by −1 returns from events with a negative predicted sign. t-statistic$_{vs0}$ assesses whether the mean return differs from 0. t-statistic$_{vs300}$ assesses whether the mean return differs from the mean return for 300 non-overlapping non-events, chosen across the sample period 2002–2005. p-value$_{bootstrap}$ is the proportion of 500 draws for which the standardized mean return across 16 randomly selected non-events exceeds the standardized mean event return. Each draw of randomly selected non-events reflects the year-by-year distribution of events.

We also conduct these tests separately on the 13 events that we assess as increasing the likelihood of IFRS adoption, and the three events that we assess as decreasing this likelihood. Disaggregating events in this fashion reduces the chance that our event returns reflect spurious market reactions to non-IFRS news because such non-IFRS news not only would have to coincide with our event dates, but also would have to be of the same sign that we predict for our event. Untabulated results tend to support the inference of a general positive (negative) investor reaction to events that increased (decreased) the likelihood of IFRS adoption. In particular, for the 13 events increasing adoption likelihood, untabulated results reveal a mean positive market-adjusted reaction of 0.0030. $\text{t-statistic}_{\text{vs0}}$ (1.896) and $\text{p-value}_{\text{bootstrap}}$ (0.054) at least marginally support the inference of a positive reaction to these 13 events. $\text{t-statistic}_{\text{vs300}}$ (1.450), however, does not support this inference. For the three events decreasing adoption likelihood, untabulated results reveal a mean market-adjusted reaction of -0.0053 that is negative, and significantly different from zero based on all three statistics ($\text{t-statistic}_{\text{vs0}} = -4.988$; $\text{t-statistic}_{\text{vs300}} = -3.760$; $\text{p-value}_{\text{bootstrap}} = 0.008$).

Collectively, the tabulated market-adjusted mean reactions are consistent with investors perceiving that the benefits to IFRS adoption outweigh the costs. However, the negative mean raw portfolio return indicates that inferences regarding investors' overall reaction to European IFRS adoption events are sensitive to whether the raw portfolio returns are market-adjusted, and potentially to the choice of index for market-adjustment. For this reason, we base our inferences regarding investors' reaction to IFRS adoption events on the cross-sectional estimation.

Cross-Sectional Analysis

Table 4, Panel A, presents descriptive statistics for the variables used in Equation (1). We estimate Equation (1) using observations for which data are available for all 16 events and for all 300 non-events (n = 31,296 firm-event observations for 1,956 firms).[28] Panel A reveals that 5.27 percent of the sample firms are banks and an average of 41.43 percent of firms' outstanding shares are held by insiders. Panel A also reveals that within-industry sales are relatively dispersed across firms (*Herf* = 0.034), 61.25 percent of sample firms are domiciled in code law countries, and 74.18 percent of firms are audited by one of the four largest auditing firms. The panel also reveals that almost 11 percent of firms have ADR listings, 24.76 percent previously applied international or U.S. GAAP, and most firms list shares on only one exchange. Table 4, Panel B, presents Pearson correlations between the variables. Consistent with our expectations, Panel B reveals that *CMAR* is significantly positively correlated with *InfoQualFactor*, *InfoQualFactor * Bank*, and *CloselyHeld*, and significantly negatively correlated with *Turnover*, *Code*, and *Size*. The correlations between *CMAR* and *Bank*, *Herf*, and *Big4* are not significantly different from zero.

Table 5 presents regression summary statistics from Equation (1), both excluding and including *Size* as a control variable. The table also presents two test statistics for each coefficient estimate. The first (in parentheses) is a t-statistic calculated using two-way clustered standard errors based on industry and country. The second (in brackets) is a t-statistic associated with a test of whether event date coefficient estimates differ from non-event date coefficient estimates.[29]

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[28] Our inferences are the same if we estimate Equation (1) using observations for which data are available for only all 16 events (n = 32,848 firm-event observations for 2,053 firms).

[29] Specifically, coefficient difference t-statistics (in brackets) are estimated for the same set of 1,956 firms using a stacked estimation of Equation (1) for the 16 event dates and for the 300 non-event dates.

TABLE 4
Descriptive Statistics

Panel A: Distributions

Variable	Mean	25%	50%	75%	Std.
Experimental Variables					
CMAR	0.0065	−0.0117	0.0059	0.0249	0.0389
InfoQualFactor	0.0002	0.3704	0.5020	0.5613	1.0001
Bank	0.0527	0.0000	0.0000	0.0000	0.2234
*InfoQualFactor * Bank*	0.0090	0.0000	0.0000	0.0000	0.2216
Turnover	0.5009	0.0000	1.0000	1.0000	0.5000
CloselyHeld	0.4143	0.2009	0.4122	0.6116	0.2572
Herf	0.0344	0.0114	0.0210	0.0422	0.0381
Code	0.6125	0.0000	1.0000	1.0000	0.4872
Big4	0.7418	0.0000	1.0000	1.0000	0.4376
Size	5.3352	3.8271	5.1880	6.7497	2.1347
Descriptive Variables					
ADR	0.1074	0.0000	0.0000	0.0000	0.3096
Standards Applied	0.2476	0.0000	0.0000	0.0000	0.4316
Exchanges	1.3405	1.0000	1.0000	1.0000	0.7973

(*continued on next page*)

TABLE 4 (continued)

Panel B: Pearson Correlations

	CMAR	*InfoQualFactor*	*Bank*	*InfoQualFactor * Bank*	*Turnover*	*CloselyHeld*	*Herf*	*Code*	*Big4*
InfoQualFactor	**0.050**								
Bank	0.003	**0.040**							
*InfoQualFactor * Bank*	**0.020**	**0.222**	**0.173**						
Turnover	**−0.028**	**0.089**	**−0.034**	**−0.020**					
CloselyHeld	**0.026**	**−0.042**	**−0.019**	**−0.019**	**−0.494**				
Herf	0.006	**−0.013**	**−0.087**	**−0.026**	**0.014**	**0.012**			
Code	**−0.021**	**−0.296**	**0.070**	0.003	**−0.317**	**0.308**	**−0.102**		
Big4	−0.001	**−0.013**	**0.103**	**0.162**	**0.162**	**−0.169**	**−0.034**	**0.061**	
Size	**−0.036**	**−0.071**	**0.200**	**0.023**	**0.203**	**−0.199**	**−0.023**	**0.213**	**0.400**

This table presents descriptive statistics for the variables used in the cross-sectional analyses. Panel A presents distributions, and Panel B presents Pearson correlations. In both panels, n = 31,296. *CMAR* is the firm's cumulative market-adjusted return, measured as the three-day return centered on the event date minus the three-day return to the DJ STOXX 1800 ex Europe Index. *InfoQualFactor* is the quality of the firm's pre-adoption information environment, measured as the highest eigenvalue factor derived from principal components analysis of the variables *ADR, Standards Applied, Exchanges,* and *Size*. *InfoQualFactor* is multiplied by −1 to ease interpretation, where higher values of *InfoQualFactor* indicate lower quality pre-adoption information. *ADR* is an indicator variable that equals 1 if a firm cross-lists in the U.S. using American Depository Receipts during the event year, and 0 otherwise. *Standards Applied* is an indicator variable equal to 1 if the firm applies U.S. standards or International Accounting Standards during the event year, and 0 if the firm applies domestic standards. *Exchanges* is the number of exchanges in which the firm lists during the event year. *Size* is the log of the firm's prior end of year market value of equity. *Bank* is an indicator variable equal to 1 if the firm's primary two-digit SIC code is 60 or 61, and 0 otherwise. *Turnover* is an indicator variable equal to 1 if the firm's mean daily percentage shares traded during the year is above the median for all firms, and 0 otherwise. *CloselyHeld* is the percentage of the firm's shares outstanding held by insiders at the end of the fiscal year. *Herf* is the Herfindahl Index, measured as the sum of each firm's squared percentage market-share, calculated at the two-digit industry level. *Code* is an indicator variable equal to 1 if the firm is domiciled in a country with a code-based legal system (all countries except the U.K. and Ireland), and 0 otherwise. *Big4* is an indicator variable equal to 1 if the firm was audited by one of the four largest accounting firms, and 0 otherwise. In Panel B, bolded values indicate significance at the 5 percent level for two-tailed tests.

TABLE 5
Cross-Sectional Analysis

Variable	Predicted Sign	Coefficient (t-statistic) [t-statistic$_{vs300}$]	Coefficient (t-statistic) [t-statistic$_{vs300}$]
Intercept	+	0.0073 (7.64) [8.33]	0.0088 (8.56) [6.15]
InfoQualFactor	+	0.0017 (5.81) [4.37]	0.0017 (5.92) [5.86]
Bank	+/−	0.0002 (0.25) [0.20]	0.0008 (1.16) [0.37]
*InfoQualFactor * Bank*	+/−	0.0017 (2.24) [2.15]	0.0017 (2.28) [2.17]
Turnover	−	−0.0023 (−3.29) [−4.92]	−0.0020 (−2.86) [−4.52]
CloselyHeld	+	0.0031 (2.57) [1.78]	0.0025 (2.10) [1.72]
Herf	+	0.0051 (0.65) [0.57]	0.0058 (0.66) [0.56]
Code	−	−0.0019 (−4.11) [−5.32]	−0.0013 (−2.43) [−4.46]
Big4	+	0.0000 (0.08) [0.00]	0.0007 (1.62) [0.24]
Size	−		−0.0005 (−3.15) [−0.05]
Firm Events		31,296	31,296
Firms		1,956	1,956
R^2		3.11%	3.15%

This table presents results from cross-sectional analyses examining the market reaction for 16 events affecting the likelihood of IFRS adoption in Europe. The estimation is an OLS regression of the following form:

$$CMAR_{j,e} = \beta_0 + \beta_1 InfoQualFactor_{j,e} + \beta_2 Bank_{j,e} + \beta_3 InfoQualFactor_{j,e} * Bank_{j,e} + \beta_4 Turnover_{j,e} + \beta_5 CloselyHeld_{j,e} + \beta_6 Herf_{j,e} + \beta_7 Code_{j,e} + \beta_8 Big4_{j,e} + \varepsilon_{j,e}.$$

CMAR is the firm's cumulative market-adjusted return, measured as the three-day return centered on the event date minus the three-day return to the DJ STOXX 1300 ex Europe Index. *InfoQualFactor* is the quality of the firm's pre-adoption information, measured as the highest eigenvalue factor derived from principal components analysis of the variables *ADR*, *Standards Applied*, *Exchanges*, and *Size*. *InfoQualFactor* is multiplied by −1 to ease interpretation, where higher values of *InfoQualFactor* indicate lower quality pre-adoption information. *ADR* is an indicator variable that equals 1 if a firm cross-lists in the U.S. using American Depository Receipts during the event year, and 0 otherwise. *Standards Applied* is an indicator variable equal to 1 if the firm applies U.S. standards or International Accounting Standards during the event year, and 0 if the firm applies domestic

(continued on next page)

TABLE 5 (continued)

standards. *Exchanges* is the number of exchanges in which the firm lists during the event year. *Size* is the log of the firm's prior end of year market value of equity. *Bank* is an indicator variable equal to 1 if the firm's primary two-digit SIC code is 60 or 61, and 0 otherwise. *Turnover* is an indicator variable equal to 1 if the firm's mean daily percentage shares traded during the year is above the median for all firms, and 0 otherwise. *CloselyHeld* is the percentage of the firm's shares outstanding held by insiders at the end of the fiscal year. *Herf* is the Herfindahl Index, measured as the sum of each firm's squared percentage market-share, calculated at the two-digit industry level. *Code* is an indicator variable equal to 1 if the firm is domiciled in a country with a code-based legal system (all countries except the U.K. and Ireland), and 0 otherwise. *Big4* is an indicator variable equal to 1 if the firm was audited by one of the four largest accounting firms, and 0 otherwise.

t-statistic is in parentheses and is the regression coefficient scaled by the coefficient standard error corrected with two-digit SIC code and country double-clustering (Petersen 2009). t-statistic$_{vs300}$ is in brackets and is derived from comparing the coefficient estimated in a regression of 1,956 firms across the 16 event dates to the coefficient estimated in a regression of 1,956 firms' 300 non-overlapping non-event dates.

Table 5 reveals that the coefficient on *InfoQualFactor*, β_1, is positive and significantly different from zero, as predicted (coefficient = 0.0017, t-statistic = 5.81). This indicates that market participants reacted more positively to IFRS adoption for firms with lower quality pre-adoption information, consistent with investors in these firms expecting greater improvements in information quality. Although the coefficient on *Bank*, β_2, is not significantly different from zero (coefficient = 0.0002, t-statistic = 0.25), the coefficient on *InfoQualFactor* * *Bank*, β_3, is positive and significantly different from zero (coefficient = 0.0017, t-statistic = 2.24).[30] This is consistent with investors in banks with lower pre-adoption information quality expecting net benefits associated with IFRS adoption.

Table 5 also reveals that the coefficient on *Turnover*, β_4, is negative and significantly different from zero (coefficient = −0.0023, t-statistic = −3.29) and the coefficient on *CloselyHeld*, β_5, is positive and significantly different from zero (coefficient = 0.0031, t-statistic = 2.57). These findings are consistent with predictions that investors in firms with higher pre-adoption information asymmetry expected a reduction in information asymmetry from adoption of IFRS. Table 5 also reveals that the coefficient on *Code*, β_7, is negative and significantly different from zero (coefficient = −0.0019, t-statistic = −4.11).[31] This finding is consistent with predictions that investors in firms domiciled in code law countries expected lower IFRS adoption benefits because of enforcement concerns.[32] The coefficients on *Herf*, β_6, and *Big4*, β_8, are not significantly different from zero (coefficients = 0.0051 and 0.0000, t-statistics = 0.65 and 0.08).

We obtain identical inferences when we compare event coefficient estimates with non-event coefficient estimates (t-statistic$_{vs300}$). This indicates that our inferences are unlikely to reflect systematic associations between the firm characteristics and market returns that might be observed on non-event dates. The second column of Table 5 also reveals that our inferences are not affected when *Size* is included as a control variable.

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[30] Untabulated findings from an analysis in which we replaced *InfoQualFactor* * *Bank* with *Size* * *Bank* reveal a significantly negative coefficient on the latter interaction variable of −0.0013 (t-statistic = −7.51), which is consistent with larger banks having an attenuated reaction to IFRS adoption. That is, smaller banks, which are more likely to have lower quality pre-adoption information, have a more positive reaction to IFRS adoption. Inferences relating to the other variables are the same as those obtained from Table 5.

[31] Even though code law firms comprise 61.25 percent of the sample and the coefficient on *Code* is significantly negative, untabulated statistics reveal that the mean market-adjusted return across the 16 events for code law firms is significantly positive.

[32] This inference may be confounded by other factors associated with code law country domicile, such as differences in institutional structure, e.g., firm governance. In Section VI, we report results of analyses aimed at assessing the sensitivity of our inference regarding enforcement concerns to alternative enforcement proxies.

Market Expectations Regarding Convergence Benefits

Evidence thus far indicates that investors reacted positively to IFRS adoption events related to expectations of both improved information quality and reduced information asymmetry. We now provide evidence on whether investor reactions also relate to expectations of convergence benefits.

To implement this analysis, we estimate Equation (1), but using dichotomous explanatory variables as follows. We define *LowInfoQualFactor* as an indicator variable equal to 1 if the firm's *InfoQualFactor* is above the sample median (that is, the firm has lower quality pre-adoption information); *LowTurnover* as an indicator variable equal to 1 if the firm has below median share turnover; *HighCloselyHeld* as an indicator variable equal to 1 if the firm has above median insider share ownership; *HighHerf* as an indicator variable equal to 1 if the firm has above median industry sales concentration distribution; *Code* as an indicator variable equal to 1 if the firm is domiciled in a code law country; *NonBig4* as an indicator variable equal to 1 if the firm does not have a Big 4 auditor; and *SmallFirm* as an indicator variable equal to 1 if the firm has below median firm size. Each variable equals 0 otherwise.

We define the variables in this way so that the intercept reflects the mean reaction for firms with high-quality information and low information asymmetry prior to IFRS adoption. That is, the intercept reflects the mean reaction for those firms with higher quality information, higher turnover, lower percentage of closely held shares, lower industry sales concentration, domiciled in common law countries, using a Big 4 auditor, and that are larger. If investors in these firms expect IFRS adoption to provide little improvement in the information quality of these firms, then any observed reaction to IFRS adoption events more likely reflects expected convergence benefits. We do not include bank variables in this estimation because we do not have predictions for industry-specific variation in expected convergence benefits.

Table 6 presents the findings. Consistent with investors expecting convergence benefits, Table 6 reveals a positive and significant market reaction for firms with higher quality information prior to the adoption of IFRS (intercept = 0.0048, t-statistic = 7.57). That is, investors appear to have anticipated benefits associated with convergence from financial reporting based on several sets of domestic accounting standards to financial reporting based on a single set of standards, i.e., IFRS, beyond any benefits associated with increased information quality. Inferences relating to the other variables in Table 6 are consistent with those we obtain from Table 5, and are not affected by including size (i.e., *SmallFirm*) as a control variable.

VI. SENSITIVITY ANALYSES

Alternative Pre-Adoption Information Quality Proxies

To assess the robustness of our inferences with respect to *InfoQualFactor*, we estimate Equation (1) separately for each variable used to derive the factor. Columns 1 through 4 of Table 7 present the results using *ADR*, *Standards Applied*, *Exchanges*, and *Size*, respectively. Consistent with Table 5, we multiply each proxy by −1 to allow us to interpret higher values as indicating lower quality pre-adoption information. For two of the variables, *Standards Applied* and *Size*, Table 7 reveals findings consistent with those in Table 5 (*Standards Applied* coefficient = 0.0041, t-statistic = 6.09; *Size* coefficient = 0.0004, t-statistic = 2.72). However, for the other two variables, the coefficients are not significantly different from zero (*ADR* coefficient = 0.0013, t-statistic = 1.50; *Exchanges* coefficient = 0.0005,

TABLE 6
Market Expectations Regarding Convergence Benefits

Variable	Predicted Sign	Coefficient (t-statistic)	Coefficient (t-statistic)
Intercept	+	0.0048 (7.57)	0.0037 (5.30)
LowInfoQualFactor	+	0.0019 (2.22)	0.0025 (3.51)
LowTurnover	+	0.0026 (3.70)	0.0024 (3.29)
HighCloselyHeld	+	0.0017 (2.91)	0.0015 (2.68)
HighHerf	+	0.0006 (0.64)	0.0006 (0.61)
Code	−	−0.0030 (−5.61)	−0.0026 (−4.18)
NonBig4	+	0.0003 (0.69)	0.0002 (0.34)
SmallFirm	+		0.0019 (2.65)
Firm Events		31,296	31,296
Firms		1,956	1,956
R^2		2.97%	3.01%

This table presents results from cross-sectional analyses examining the market reaction for 16 events affecting the likelihood of IFRS adoption in Europe. The estimation is an OLS regression of the following form:

$$CMAR_{j,e} = \gamma_0 + \gamma_1 LowInfoQualFactor_{j,e} + \gamma_2 LowTurnover_{j,e} + \gamma_3 HighCloselyHeld_{j,e} + \gamma_4 HighHerf_{j,e} + \gamma_5 Code_{j,e} + \gamma_6 NonBig4_{j,e} + \tau_{j,e}.$$

CMAR is the firm's cumulative market-adjusted return, measured as the three-day return centered on the event date minus the three-day return to the DJ STOXX 1800 ex Europe Index. *LowInfoQualFactor* is a dichotomous variable that equals 1 if *InfoQualFactor* is at or above the distribution median (indicating low pre-adoption information quality), and equals 0 otherwise. *InfoQualFactor* is the quality of the firm's pre-adoption information, measured as the highest eigenvalue factor derived from principal components analysis of the variables *ADR*, *Standards Applied*, *Exchanges*, and *Size*. *InfoQualFactor* is multiplied by −1 to ease interpretation, where higher values of *InfoQualFactor* indicate lower quality pre-adoption information. *ADR* is an indicator variable that equals 1 if a firm cross-lists in the U.S. using American Depository Receipts (ADR) during the event year, and 0 otherwise. *Standards Applied* is an indicator variable equal to 1 if the firm applies U.S. standards or International Accounting Standards during the event year, and 0 if the firm applies domestic standards. *Exchanges* is the number of exchanges in which the firm lists during the event year. *Size* is the log of the firm's prior end of year market value of equity. *LowTurnover* is an indicator variable equal to 1 if the firm's mean daily percentage shares traded during the year is below the median for all firms, and 0 otherwise. *HighCloselyHeld* is a dichotomous variable that equals 1 if the percentage of the firm's shares outstanding held by insiders at the end of the fiscal year is at or above the distribution median, and 0 otherwise. *HighHerf* is a dichotomous variable that equals 1 if the sum the firm's squared percentage market-share is at or above the distribution median of the Herfindahl index, calculated at the two-digit industry level, and 0 otherwise. *Code* is an indicator variable equal to 1 if the firm is domiciled in a country with a code-based legal system (all countries except the U.K. and Ireland), and 0 otherwise. *NonBig4* is an indicator variable equal to 1 if the firm was not audited by one of the four largest accounting firms, and 0 otherwise. *SmallFirm* is a dichotomous variable that equals 1 if *Size* is below the distribution median, and 0 otherwise.

t-statistic is in parentheses and is the regression coefficient scaled by the coefficient standard error corrected with two-digit SIC code and country double-clustering (Petersen 2009).

TABLE 7
Alternative Information Quality Proxies

		Proxy for Pre-Adoption Information Quality (*InfoQualProxy*)			
Variable	**Predicted Sign**	***ADR***	***Standards Applied***	***Exchanges***	***Size***
Intercept	+	0.0079	0.0081	0.0083	0.0092
		(7.27)	(8.22)	(7.75)	(6.49)
InfoQualProxy	+	0.0013	0.0041	0.0005	0.0004
		(1.50)	(6.09)	(1.61)	(2.72)
Bank	+/−	0.0009	0.0015	0.0029	0.0081
		(1.26)	(2.07)	(3.34)	(4.82)
*InfoQualProxy * Bank*	+/−	−0.0001	0.0047	0.0014	0.0009
		(−0.02)	(2.65)	(3.12)	(3.84)
Turnover	−	−0.0022	−0.0023	−0.0023	−0.0019
		(−3.19)	(−3.30)	(−3.37)	(−2.88)
CloselyHeld	+	0.0033	0.0029	0.0034	0.0029
		(2.43)	(2.48)	(2.62)	(1.98)
Herf	+	0.0032	0.0049	0.0027	0.0034
		(0.38)	(0.62)	(0.33)	(0.36)
Code	−	−0.0029	−0.0017	−0.0028	−0.0024
		(−5.05)	(−3.46)	(−5.21)	(−3.41)
Big4	+	0.0001	0.0001	0.0001	0.0007
		(0.27)	(0.32)	(0.20)	(1.44)
Firm Days		31,296	31,296	31,296	31,296
Firms		1,956	1,956	1,956	1,956
R^2		2.92%	3.13%	2.93%	2.97%

This table presents results from cross-sectional analyses examining the market reaction for 16 events affecting the likelihood of IFRS adoption in Europe. The estimation is an OLS regression of the following form:

$$\begin{aligned} CMAR_{j,e} = {} & \delta_0 + \delta_1 InfoQualProxy_{j,e} + \delta_2 Bank_{j,e} + \delta_3 InfoQualProxy_{j,e} * Bank_{j,e} + \delta_4 Turnover_{j,e} \\ & + \delta_5 CloselyHeld_{j,e} + \delta_6 Herf_{j,e} + \delta_7 Code_{j,e} + \delta_8 Big4_{j,e} + \upsilon_{j,e}. \end{aligned}$$

CMAR is the firm's cumulative market-adjusted return, measured as the three-day return centered on the event date minus the three-day return to the DJ STOXX 1800 ex Europe Index. *InfoQualProxy* is the proxy for the quality of the firm's pre-adoption information, alternatively measured as *ADR, Standards Applied, Exchanges,* and *Size*. Each proxy is multiplied by −1 to ease interpretation (implying higher values indicate lower quality pre-adoption information). *ADR* is an indicator variable that equals 1 if a firm cross-lists in the U.S. using American Depository Receipts during the event year, and 0 otherwise. *Standards Applied* is an indicator variable equal to 1 if the firm applies U.S. standards or International Accounting Standards during the event year, and 0 if the firm applies domestic standards. *Exchanges* is the number of exchanges in which the firm lists during the event year. *Size* is the log of the firm's prior end of year market value of equity. *Bank* is an indicator variable equal to 1 if the firm's primary two-digit SIC code is 60 or 61, and 0 otherwise. *Turnover* is an indicator variable equal to 1 if the firm's mean daily percentage shares traded during the year is above the median for all firms, and 0 otherwise. *CloselyHeld* is the percentage of the firm's shares outstanding held by insiders at the end of the fiscal year. *Herf* is the Herfindahl Index, measured as the sum of each firm's squared percentage market-share, calculated at the two-digit industry level. *Code* is an indicator variable equal to 1 if the firm is domiciled in a country with a code-based legal system (all countries except the U.K. and Ireland), and 0 otherwise. *Big4* is an indicator variable equal to 1 if the firm was audited by one of the four largest accounting firms, and 0 otherwise.

t-statistic is in parentheses and is the regression coefficient scaled by the coefficient standard error corrected with two-digit SIC code and country double-clustering (Petersen 2009).

t-statistic = 1.61). Table 7 also reveals that the coefficients on *Bank* and *InfoQualProxy* * *Bank* are positive and significantly different from zero for *Standards Applied, Exchanges,* and *Size*, but not for *ADR*.[33] Inferences relating to the other variables are the same as those obtained from Table 5.

Evidence in Table 7 highlights the difficulty in isolating a single underlying variable that sufficiently captures pre-adoption information quality. Creating a factor by using the four variables together to characterize a firm's information quality, as in the estimation presented in Table 5, appears to improve our ability to capture this construct and, thus, increase the power of our test.

Alternative Enforcement Environment Proxies

Our primary cross-sectional analyses rely on the variable *Code* to assess whether the reaction to IFRS adoption differs for firms domiciled in code law and common law countries. We interpret *Code* as capturing the effect of weaker enforcement on expected benefits associated with IFRS adoption because the estimation equation includes controls for other firm characteristics, including the quality of pre-adoption information. Nonetheless, it is possible that *Code* also captures other firm characteristics, including some related to the firm's pre-adoption information quality. Thus, to assess the robustness of our inferences related to enforcement, we re-estimate Equation (1) using three alternative enforcement proxies.

The alternative proxies are *Rule of Law, Control of Corruption,* which we obtain from Kaufmann et al. (2007), and the average of these two variables, *Average Enforcement.*[34] *Rule of Law* is an annual country-specific measure of contract enforcement quality, and police and court system quality. *Control of Corruption* is an annual country-specific measure of the extent to which public power is exercised for private gain and the degree of capture of the state by private interests. We multiply the Kaufmann et al. (2007) variables by −1 so that larger values indicate weaker enforcement environments, analogous to the way we define *Code.* Consistent with enforcement being weaker in code law countries, untabulated findings reveal that the correlation between *Average Enforcement* and *Code* is positive, 0.333, and significantly different from zero.

Table 8 presents the findings and reveals results consistent with those in Table 5. Specifically, the coefficients on all three enforcement variables are negative and marginally significantly different from zero: *Rule of Law* (coefficient = −0.0017, t-statistic = −1.84), *Control of Corruption* (coefficient = −0.0013, t-statistic = −1.70), and *Average Enforcement* (coefficient = −0.0015, t-statistic = −1.76). Inferences relating to the other variables are similar to those obtained from Table 5. Collectively, the findings in Table 8 support the inference that the market reaction is less positive for firms domiciled in jurisdictions likely to have weaker enforcement.

VII. CONCLUSION

This study investigates the European equity market reaction to 16 events associated with the adoption of IFRS in Europe. IFRS adoption resulted in a broad cross-section of firms domiciled in European countries with a variety of domestic accounting standards changing to a common set of standards at the same time. The prospects of IFRS adoption

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[33] The lack of significance for the *ADR* * *Bank* coefficient could result from the small number of observations underlying this coefficient estimation. In particular, only 12 banks in the sample have ADR listings.

[34] We use the Kaufmann et al. (2007) variables, rather than those provided by La Porta et al. (1998), because the Kaufmann et al. (2007) variables vary over time and align better in calendar time with our study than do the non-time-varying La Porta et al. (1998) measures.

TABLE 8
Alternative Enforcement Environment Proxies

		Proxy for Enforcement Environment (*Enf_Environ*)		
Variable	**Predicted Sign**	***Rule of Law***	***Control of Corruption***	***Average Enforcement***
Intercept	+	0.0034	0.0038	0.0036
		(1.86)	(2.48)	(2.17)
InfoQualFactor	+	0.0020	0.0020	0.0020
		(7.31)	(7.17)	(7.23)
Bank	+/−	0.0003	0.0003	0.0003
		(0.40)	(0.41)	(0.41)
*InfoQualFactor * Bank*	+/−	0.0014	0.0014	0.0014
		(1.82)	(1.85)	(1.84)
Turnover	−	−0.0019	−0.0019	−0.0019
		(−2.69)	(−2.75)	(−2.72)
CloselyHeld	+	0.0028	0.0028	0.0028
		(2.38)	(2.47)	(2.44)
Herf	+	0.0071	0.0067	0.0069
		(0.89)	(0.85)	(0.86)
Enf_Environ	−	−0.0017	−0.0013	−0.0015
		(−1.84)	(−1.70)	(−1.76)
Big4	+	−0.0001	−0.0002	−0.0001
		(−0.33)	(−0.34)	(−0.34)
Firm Days		31,296	31,296	31,296
Firms		1,956	1,956	1,956
R^2		3.09%	3.09%	3.09%

This table presents results from cross-sectional analyses examining the market reaction for 16 events affecting the likelihood of IFRS adoption in Europe. The estimation is an OLS regression of the following form:

$$CMAR_{j,e} = \theta_0 + \theta_1 InfoQualFactor_{j,e} + \theta_2 Bank_{j,e} + \theta_3 InfoQualFactor_{j,e} * Bank_{j,e} + \theta_4 Turnover_{j,e}$$
$$+ \theta_5 CloselyHeld_{j,e} + \theta_6 Herf_{j,e} + \theta_7 Enf\_Environ_{j,e} + \theta_8 Big4_{j,e} + \omega_{j,e}.$$

CMAR is the firm's cumulative market-adjusted return, measured as the three-day return centered on the event date minus the three-day return to the DJ STOXX 1800 ex Europe Index. *InfoQualFactor* is the quality of the firm's pre-adoption information, measured as the highest eigenvalue factor derived from principal components analysis of the variables *ADR*, *Standards Applied*, *Exchanges*, and *Size*. *InfoQualFactor* is multiplied by −1 to ease interpretation, where higher values of *InfoQualFactor* indicate lower quality pre-adoption information. *ADR* is an indicator variable that equals 1 if a firm cross-lists in the U.S. using American Depository Receipts during the event year, and 0 otherwise. *Standards Applied* is an indicator variable equal to 1 if the firm applies U.S. standards or International Accounting Standards during the event year, and 0 if the firm applies domestic standards. *Exchanges* is the number of exchanges in which the firm lists during the event year. *Size* is the log of the firm's prior end of year market value of equity. *Bank* is an indicator variable equal to 1 if the firm's primary two-digit SIC code is 60 or 61, and 0 otherwise. *Turnover* is an indicator variable equal to 1 if the firm's mean daily percentage shares traded during the year is above the median for all firms, and 0 otherwise. *CloselyHeld* is the percentage of the firm's shares outstanding held by insiders at the end of the fiscal year. *Herf* is the Herfindahl Index, measured as the sum of each firm's squared percentage market-share, calculated at the two-digit industry level. *Enf_Environ* is one of three proxies for the firm's enforcement environment, derived annually at the country level from Kaufmann et al. (2007). To ease interpretation, the Kaufmann et al. (2007) measures are multiplied by −1 so that larger values indicate weaker enforcement. *Rule of Law* measures the quality of contract enforcement, the police, and the courts. *Control of Corruption* measures the extent to which public power is exercised for private gain, including both petty and grand forms of corruption, as well as "capture" of the state by elites and private interests. *Average Enforcement* is the average of these two variables.

(*continued on next page*)

TABLE 8 (continued)

Big4 is an indicator variable equal to 1 if the firm was audited by one of the four largest accounting firms, and 0 otherwise.

t-statistic is in parentheses and is the regression coefficient scaled by the coefficient standard error corrected with two-digit SIC code and country double-clustering (Petersen 2009).

led investors in European firms to assess the implications of potential changes in firms' information environments and convergence associated with this change in financial reporting standards. Thus, events leading to IFRS adoption in Europe provide an opportunity to assess investors' expectations about the net benefits or net costs of IFRS adoption.

We find an incrementally positive reaction for European firms with lower pre-adoption information quality and higher pre-adoption information asymmetry. These findings are consistent with investors expecting IFRS to improve the information quality for these firms. We find an even more positive reaction for banks with lower pre-adoption information quality, which is consistent with investors expecting improvements in information quality—including any associated with adoption of the controversial IAS 39—for these firms. We find an incrementally negative reaction for firms domiciled in code law countries, which are likely to have weaker enforcement of accounting standards. Regarding expected convergence benefits, we find a positive reaction to IFRS adoption events even for firms with high-quality pre-adoption information. To the extent investors expect IFRS adoption to affect only minimally the information of these firms, this finding is consistent with investors expecting net benefits associated with convergence from IFRS adoption.

Overall, our findings suggest that investors expected net benefits to IFRS adoption in Europe associated with increases in information quality, decreases in information asymmetry, more rigorous enforcement of the standards, and convergence. We leave it to future research to determine whether these expectations were fulfilled.

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Managers' EPS Forecasts: Nickeling and Diming the Market?

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ABSTRACT: Nearly half of managers' forecasts of annual earnings per share (EPS) end in nickel intervals, whereas only about 20 percent of actual EPS end in nickel intervals. We provide evidence on the attributes, determinants, and consequences of this systematic wedge between managers' predictions and firms' *ex post* actual performance. Managers' nickel forecasts are not simply a benign response to uncertainty about upcoming earnings, because nickel forecasts are not only less accurate, but also they are more optimistically biased than non-nickel forecasts. In addition to uncertainty, efforts to protect the firm's proprietary information and self-serving opportunism in response to managers' economic incentives also play incremental roles in explaining managers' propensity to issue forecasts heaped at nickel intervals. We also find that managers' nickel forecasts spur even active analysts to issue forecasts heaped at nickel intervals, although analysts' forecast revisions partially adjust for the optimism and noise in managers' nickel forecasts.

Keywords: *voluntary disclosure; management earnings forecasts; heaping; proprietary information costs.*

Data Availability: *Data are available from sources identified in the text.*

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I. INTRODUCTION

This study identifies a new systematic difference between managers' predictions about their firms' earnings performance and the firms' *ex post* realized results: when managers voluntarily issue forecasts of annual earnings per share (EPS), nearly *half* of those forecasts end in nickel intervals (i.e., an "even" dollar, a half-dollar, a quarter, a dime, or a nickel amount), whereas only about 20 percent of actual EPS end in a nickel interval.[1] The tendency to provide estimates ending in digits that are the largest divisors of the base of the number system is referred to as "heaping" (Turner 1958) in a fairly extensive literature lying largely outside accounting.

We propose and test economic determinants of this wedge between managers' forecasts and their firms' actual earnings. Prior research suggests heaping is a heuristic response to uncertainty (e.g., Huttenlocher et al. 1990) that arises when people make point estimates about phenomena about which they lack precise information, such as elapsed time, height, or weight. In the only published study of heaping in the accounting literature, Herrmann and Thomas (2005) show that *analysts'* forecasts of EPS heap at nickel intervals, and this heaping in *analysts'* forecasts is largely attributable to analysts who are less informed and thus have more uncertainty about upcoming earnings. Uncertainty is unlikely to be the sole determinant of heaping in *management* forecasts, however. Managers have more precise private information, and they are not required to issue point forecasts of earnings—both of which should reduce heaping—but surprisingly, we find that managers' forecasts are only slightly less likely to heap at nickel intervals than analysts' forecasts. Moreover, we find that managers' nickel forecasts are not only less accurate, but they are also more optimistically biased than their non-nickel forecasts,[2] so managers' nickel forecasts are not simply benign responses to uncertainty (as the extant heaping literature proposes), because random error would lead to less accurate but unbiased forecasts.

Consequently, we must look beyond the heaping literature to explain heaping in *managers'* earnings forecasts. We consider how managers' strategic economic incentives may spur heaping. First, theory suggests that firms with higher proprietary information costs reveal less information (Verrecchia 1983). Heaped estimates are on average noisier than nonheaped estimates, so we expect that when firms with higher proprietary information costs choose to issue forecasts, those forecasts are more likely to heap at nickel intervals. Second, we expect managers with greater *ex ante* economic incentives to issue optimistic forecasts will be more likely to issue forecasts that heap, if those managers believe market participants interpret "rounded" estimates as suggesting managers are more uncertain about earnings, which in turn reduces the likelihood that market participants attribute error in the manager's forecasts to intentional misrepresentation.

Our results are consistent with our hypotheses: management forecasts are more likely to heap at nickel intervals not only when there is more uncertainty about the firm's earnings, but also when the firm has higher proprietary information costs and managers have stronger economic incentives to upward-bias their forecasts. These results suggest that heaping in management earnings forecasts stems not only from benign psychological heuristic responses to elevated uncertainty, but also from strategic incentives including an effort to protect the firm's proprietary information and managers' self-serving opportunism in response to their economic incentives.

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[1] Hereafter, we refer to managers' forecasts ending in nickel intervals as "nickel forecasts."

[2] Managers' propensity to issue biased forecasts has changed over time. Forecasts issued in the 1960s and 1970s tended to be optimistically biased (e.g., McDonald 1973; Basi et al. 1976; Penman 1980). During the more litigious 1980s, forecasts were on average unbiased (e.g., McNichols 1989; Frankel et al. 1995). Studies after the 1995 Private Securities Litigation Reform Act reduced managers' liability for forward-looking disclosures generally find that managers' forecasts of annual earnings are optimistically biased (e.g., Choi and Ziebart 2004; Rogers and Stocken 2005; Ajinkya et al. 2005; Bergman and Roychowdhury 2008). Although our sample of management forecasts of annual earnings are optimistic relative to *ex post* actual earnings, on average they convey bad news relative to market expectations (i.e., downward guidance), similar to prior research (e.g., Bamber and Cheon 1998).

Finally, we examine consequences of heaping in management forecasts. Herrmann and Thomas (2005) conclude that *analysts* with fewer resources or lower ability are more likely to heap, whereas active analysts who follow the firm on a timely basis are less likely to heap. We find that management forecasts heaped at nickel intervals increase heaping even among active analysts. Further tests suggest that analysts make some adjustments for the lower accuracy of managers' nickel forecasts—at least for nickel forecasts that convey good news. This adjustment is incomplete, however, because analysts' forecasts remain more optimistically biased and less accurate after nickel than non-nickel management forecasts.

Our study makes several contributions. First, evidence that heaping in managers' earnings forecasts is partially attributable to a heuristic response to uncertainty is, to our knowledge, the first archival evidence that managers' voluntary financial disclosures appear to reflect psychological heuristics. Second, whereas extant literature views heaping as a benign heuristic response to uncertainty, we identify a new and fundamentally different driver of heaping by demonstrating that managers' strategic incentives help explain heaping. Specifically, we show that heaping is associated with an apparent effort to protect the firm's proprietary information, and with managers' strategic incentives and opportunities to issue biased disclosures. To our knowledge, this is the first archival evidence that strategic incentives (as distinct from subconscious heuristics) help explain heaping and, as such, we contribute to the heaping literature more generally, in addition to the management forecast literature more specifically.

Third, in a recent review, Hirst et al. (2008, 334) conclude that the voluntary disclosure literature has focused on identifying determinants and consequences of managers' decisions whether to issue forecasts; yet "managers' choice of forecast characteristics [e.g., precision, accuracy, and bias] appears to be the least understood (both in terms of theory and research) even though it is the component over which managers have the most control." Our study helps address this void in the literature by identifying a new element of the specificity or precision of management forecasts, along with evidence on the determinants and consequences of this choice. Specifically, our evidence that heaped forecasts are noisier and more upward-biased than nonheaped forecasts, and that heaping appears to be, in part, a response to managers' strategic economic incentives, suggests the choice between nickel and non-nickel forecasts is an additional, finer, element of the forecast precision choice that has not been previously identified. Because this evidence suggests that "not all point forecasts are created equal," future research may benefit from refining the traditional categories of forecast precision by delineating nickel versus non-nickel forecasts rather than categorizing all point forecasts as if they are equally precise (e.g., Baginski and Hassell 1997; Bamber and Cheon 1998; Baginski et al. 2004).

Fourth, while Herrmann and Thomas (2005) suggest that heaping in *analysts'* forecasts is largely attributable to analysts who are less informed and that more informed analysts who follow the firm closely are less likely to heap, our evidence shows that heaped management forecasts spur heaping even among active analysts who follow the firm on a timely basis. Finally, our evidence that analysts partially, but not fully, adjust for the predictable bias and additional noise in managers' nickel forecasts is of interest from a practical perspective.

II. HYPOTHESIS DEVELOPMENT

Frequency of Managers' Nickel Forecasts

Research in psychology, demography, and other fields suggests that humans' quantitative responses tend to end in common intervals (e.g., Yerkes and Urban 1906; Myers 1954; Turner 1958; Stockwell 1966; Stockwell and Wicks 1974; Huttenlocher et al. 1990; Rowland 1990; Whynes et al. 2005). Turner (1958) observes that humans tend to give responses that end in digits that are the largest divisors of the base of the number system. Since the base-ten system has ten and five as the largest divisors, humans' estimates commonly end in multiples of ten and five. Turner (1958) refers to this phenomenon as "heaping." This literature interprets heaping as a heuristic response to uncertainty that arises when people (1) make point estimates about (2) phenomena about which they lack precise information. Consequently, heaped responses are typically less accurate than nonheaped responses (e.g., Huttenlocher et al. 1990; Rowland 1990).

In an accounting context, Herrmann and Thomas (2005) show that *financial analysts'* EPS forecasts tend to heap at nickel intervals (i.e., multiples of five or ten).[3] They further show that *analysts'* heaping stems from uncertainty arising from lack of informedness (e.g., proxies for resources the analyst can devote to following the firm, such as broker size, the number of other firms the analyst follows, the number of other industries the analyst follows, and the frequency with which the analyst issues forecasts for that firm). Finally, Herrmann and Thomas (2005) show that better-informed analysts are less likely to heap.

In contrast to Herrmann and Thomas (2005), we investigate heaping in *managers'* earnings forecasts. We begin by benchmarking the pervasiveness of managers' heaping relative to heaping in analysts' forecasts. The fact that managers have more private information about future firm performance than analysts suggests managers' forecasts should exhibit less heaping. Moreover, unlike analysts, managers who are uncertain about upcoming earnings need not issue point forecasts of EPS. Managers can issue range or open-ended forecasts, they can forecast revenues, or they can remain silent. Thus, managers' superior information and flexibility to avoid issuing point forecasts should attenuate heaping.

On the other hand, as we later explain in more detail, managers may have strategic incentives to heap—incentives that do not apply to analysts. Managers who want to adjust the market's expectation, but whose firms have higher proprietary information costs, may issue heaped forecasts (that are on average noisier and thus less informative). Managers' incentives to issue biased forecasts can provide another motivation to heap. Rogers and Stocken (2005) argue that managers are more likely to issue biased forecasts when uncertainty about earnings makes it difficult for market participants to determine *ex post* whether the forecast was in fact a biased representation of managers' private information. When there is more uncertainty about earnings, market participants understand that managers' forecasts will contain more error, so they are less likely to attribute errors in management forecasts to deliberate misrepresentation. To the extent that market participants view heaped forecasts as "rounded" estimates indicating greater uncertainty about the firm's earnings (ancillary analysis reported in Section VI supports this view), they are less likely to attribute errors in heaped forecasts to intentional managerial bias. Thus, managers with stronger incentives to issue biased forecasts may be more likely to issue heaped forecasts in an effort to escape detection.

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[3] A recent working paper, Zhou (2006) also finds that analysts' forecasts heap at nickel intervals.

In summary, managers are not required to issue point forecasts of earnings, and they face less uncertainty about upcoming earnings, both of which should attenuate heaping. On the other hand, managers likely have strategic incentives (that do not apply to analysts) that could exacerbate heaping. We therefore do not specify a directional prediction on the extent of heaping in managers' EPS forecasts relative to analysts' forecasts.

H1: Managers' forecasts of annual EPS are equally likely to heap at nickel intervals as are analysts' forecasts.

Characteristics of Managers' Nickel Forecasts

As explained earlier, prior literature suggests heaping is a heuristic response to uncertainty, which suggests heaped management forecasts will be less accurate than nonheaped forecasts.[4]

H2a: Managers' forecasts of annual EPS heaped at nickel intervals are less accurate than their nonheaped/non-nickel forecasts.

An intriguing study in the medical literature also concludes that heaped estimates are more biased. Rowland (1990) shows that respondents' estimates of their weight heap at intervals ending in zero and five. Consistent with motivated reasoning theory and evidence (e.g., Kunda 1990; Hales 2007), he also finds that a subset of the heaped responses are more biased than the nonheaped responses in favor of respondents' preferred outcomes. (Specifically, among overweight females, those whose estimates heaped at 0 or 5 underreported weight to a greater extent than those who provided nonheaped estimates.) The implication for our context is that managers' heaped nickel forecasts may be more biased than their nonheaped non-nickel forecasts.

Managers' forecasts of annual earnings are longer-horizon forecasts that are, on average, optimistically biased (e.g., Choi and Ziebart 2004; Rogers and Stocken 2005; Ajinkya et al. 2005).[5] Bergman and Roychowdhury (2008, 1057) explain: "firms' long-horizon disclosure choices reflect managers' desire to maintain optimistic earnings valuations," consistent with Hales' (2007, 609) evidence that "people are likely to become optimistic about the outcomes of things they care about." Given that managers' forecasts of annual EPS tend to be optimistic, we expect any differential bias in managers' heaped nickel forecasts to reflect greater optimism.

H2b: Managers' nickel forecasts of annual EPS are more optimistically biased than their non-nickel forecasts.

Determinants of Managers' Nickel Forecasts

Prior literature views heaping as a heuristic response to uncertainty (e.g., Turner 1958; Huttenlocher et al. 1990). It is therefore straightforward to predict:

H3a: A manager's forecast of annual EPS is more likely to heap at a nickel interval when the manager is more uncertain about future earnings.

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[4] Managers' strategic incentives to heap (to protect proprietary information or to "hide" biased forecasts via rounded estimates) could also lead heaped forecasts to be less accurate.

[5] In contrast, short-horizon quarterly forecasts are generally issued to set pessimistic, beatable expectations (Hirst et al. 2008).

We extend beyond extant literature in hypothesizing that two aspects of managers' strategic economic incentives may spur heaping in their forecasts. First, theory suggests that managers of firms with higher proprietary information costs disclose less, *ceteris paribus* (e.g., Verrecchia 1983), although archival evidence on this theoretical proposition is fairly limited (e.g., Bamber and Cheon 1998; Botosan and Stanford 2005; Wang 2007). Our analysis is necessarily conditioned on managers choosing to issue an earnings forecast, but we expect cross-sectional variation in proprietary information costs within the set of forecasting firms will help explain heaping.[6] Specifically, among managers who decide to issue a forecast, we expect those whose firms have higher proprietary information costs will be more likely to issue heaped nickel forecasts (that are on average nosier and thus less informative):

H3b: A manager's forecast of annual EPS is more likely to heap at a nickel interval when the firm has higher proprietary information costs.

Finally, as explained earlier, we expect managers' nickel forecasts to be more optimistically biased than their non-nickel forecasts. We therefore expect managers' propensity to issue forecasts that heap at nickel intervals is partially attributable to their *ex ante* incentives to upwardly bias their forecasts. Rogers and Stocken (2005) argue that managers are more likely to issue biased forecasts when: (1) they have economic incentives to do so, and (2) it is difficult for market participants to assess the truthfulness of the forecasts (i.e., *ex post* settling up is noisy because uncertainty about earnings makes it difficult to ascertain whether the forecast was in fact a biased representation of the manager's private information). Market participants likely view heaped nickel forecasts as noisier "rounded" estimates, indicative of greater uncertainty about earnings, which in turn makes it less likely they will attribute forecast error to intentional misrepresentation. We therefore hypothesize that managers attempt to strategically "hide" optimistic bias by issuing heaped nickel forecasts that complicate *ex post* settling up.

H3c: A manager's forecast of annual EPS is more likely to heap at a nickel interval when the manager has greater incentive to optimistically bias expectations.

Consequences of Managers' Nickel Forecasts

The expectations adjustment hypothesis posits that managers issue EPS forecasts to align the market's expectations of earnings with managers' expectations (Ajinkya and Gift 1984). We therefore focus on how managers' forecasts affect analysts' forecasts of the same EPS number. The revision in analysts' forecasts directly measures the change in expectations about current period earnings attributable to the management forecast, so our tests are not confounded by the effect of the management forecast on expectations about earnings beyond the current period or differences in earnings capitalization rates, both of which complicate analysis of price reactions to forecasts (Yeung 2009).

First, we expect that if management issues a nickel forecast, this will spur analysts to heap their forecasts at nickel intervals. That is, we hypothesize that part of the heaping Herrmann and Thomas (2005) observe in analysts' forecasts stems from heaping in managers' forecasts:

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[6] Ancillary analysis confirms that firms with the highest proprietary information costs tend to avoid forecasting. Nonforecasting firms (that meet our other sample criteria) have higher proprietary information costs than our sample firms that have all chosen to issue forecasts ($p \leq 0.02$).

H4a: When managers issue nickel forecasts, analysts' subsequent forecasts are more likely to heap at nickel intervals.

Second, we investigate whether *analysts* adjust for the lower accuracy of *managers'* nickel forecasts. Herrmann and Thomas (2005) conclude that *share prices* do not adjust for the lower accuracy of *analysts'* heaped forecasts. In contrast, we expect *analysts* adjust for the lower accuracy of *managers'* nickel forecasts because: (1) analysts' forecasts may reflect more sophisticated earnings expectations than prices (e.g., Abarbanell and Bernard 1992; Walther 1997; Bloomfield 2002), and (2) as explained above, analysts' forecasts of the same earnings number provide more direct (less noisy) evidence on how management forecasts affect expectations about current earnings.

H4b: Analysts' revisions of their EPS forecasts adjust for the lower accuracy of managers' nickel forecasts relative to their non-nickel forecasts.

III. SAMPLE SELECTION AND DESCRIPTION

We collect all management point forecasts of annual EPS from First Call's Company Issued Guidelines (CIG) database for the fiscal years 1996–2004.[7] Our analysis is conditioned on management's decision to issue a point forecast of annual earnings—we focus on whether the forecast ends in a nickel interval or a non-nickel interval.[8] To isolate earnings forecasts rather than preannouncements, we eliminate forecasts issued after the year-end. To compare the incidence of heaping in management versus analysts' forecasts, we retain management forecasts for firm-years that also have at least one analyst forecast. Our analysis uses actual (unadjusted) analyst and management forecast data to avoid the rounding issues arising in split-adjusted data documented in Payne and Thomas (2003). These criteria yield 3,265 management point forecasts of annual EPS.

We begin by providing evidence on the frequency (H1) and characteristics (H2) of heaping in this population of 3,265 forecasts. Analyses of the determinants (H3) and consequences (H4) of heaping are based on the subset of 1,510 forecasts with necessary CRSP, Compustat, I/B/E/S, and NYSE Trade and Quote (TAQ) data, as shown in Panel A of Table 1. Panel B shows that this subsample is representative of the population in terms of firm size. Panel C shows that the number of forecasts increases over the 1996–2004 period. Panel D shows that the distribution of firms across major two-digit SIC code industry groups is similar to that of firms in the intersection of CRSP/Compustat/I/B/E/S/TAQ/First Call databases.

IV. FREQUENCY AND PROPERTIES OF MANAGERS' NICKEL FORECASTS

Frequency of Managers' Nickel Forecasts

Panel A of Figure 1 shows the relative frequency of the last two digits of managers' EPS forecasts, measured as the number of management forecasts in each last-two-digit EPS interval, divided by the total number of forecasts. Panel B shows the relative frequency of the last two digits of actual EPS for these same firm-years (as reported by First Call for

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[7] Ajinkya et al. (2005, 350) conclude that CIG is a comprehensive source of management forecasts, after performing a small-sample test (in 1997 and in 2000) matching CIG forecasts with those identified in a keyword search of Factiva (formerly the Dow Jones News Retrieval Service). Choi and Ziebart (2004) also report that CIG provides a more complete set of management forecasts than the Dow Jones News Retrieval System over the period 1993–1996.

[8] Point forecasts provide the cleanest setting for testing our hypotheses, because measurement of forecast bias and accuracy is unambiguous. As explained in Section VI, however, the characteristics, determinants, and consequences of heaping in range management forecasts are similar to those we document for point forecasts.

TABLE 1
Sample of Management's Point Forecasts of Annual Earnings Per Share

Panel A: Sample Selection

	Number of Management Forecasts	Number of Firms
First Call point forecasts of annual EPS issued by managers, for firms with I/B/E/S analyst following for fiscal years 1996–2004[a] (H1 and H2)	3,265	1,149
Forecasts with CRSP, Compustat, I/B/E/S data available	1,783	600
Forecasts with TAQ data available (H3 and H4)	1,510	530

Panel B: Comparison of Market Value

	Median Market Value (in $ millions)		
	Population (n = 3,265)	Final Sample (n = 1,510)	Wilcoxon z
NYSE	2,675	2,734	(−0.08)
AMEX	108	131	(−0.32)
NASDAQ	883	932	(−0.56)

Panel C: Number of Forecasts Issued for Each Fiscal Year

Year	Number of Forecasts	% of Sample
1996	58	3.84
1997	81	5.37
1998	134	8.87
1999	127	8.41
2000	147	9.74
2001	158	10.46
2002	256	16.95
2003	272	18.01
2004	277	18.35
Total	1,510	100.00

Panel D: Industry Composition

Two-Digit SIC Industry Groups	% of Sample Firms	% of CRSP/Compustat I/B/E/S/TAQ/First Call Firms
Agriculture and Forestry (01–09)	0.13	0.26
Mining (10–14)	2.12	2.68
Construction (15–17)	2.05	1.38
Manufacturing (20–39)	54.64	51.51
Telecommunication (48)	1.19	2.55
Wholesale (50–51)	2.19	3.89
Retail (52–59)	14.64	11.77
Services (70–88)	22.91	25.74
Other	0.13	0.22
	100.00	100.00

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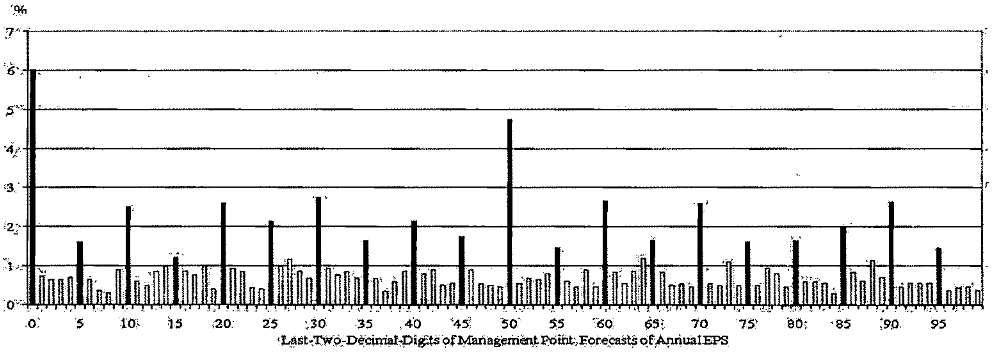
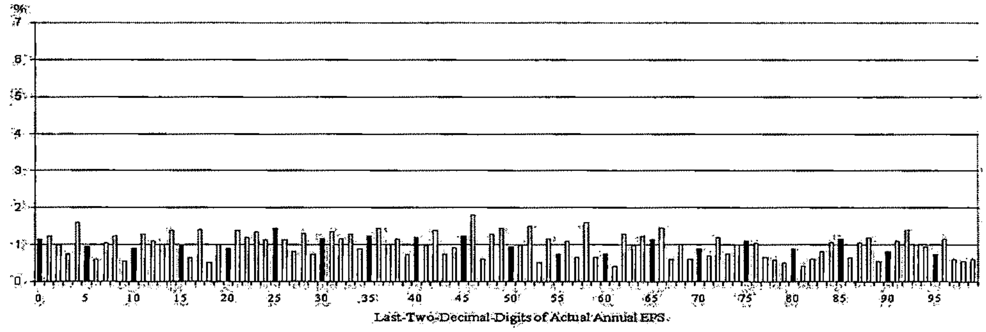
[a] Point management forecasts of annual EPS, collected by First Call (Forecast type = "A," "F," and "Z") issued prior to the fiscal year-end.

FIGURE 1
Relative Frequencies of Last Two Digits of Management Forecasts, Actual EPS, and Analysts' Forecasts

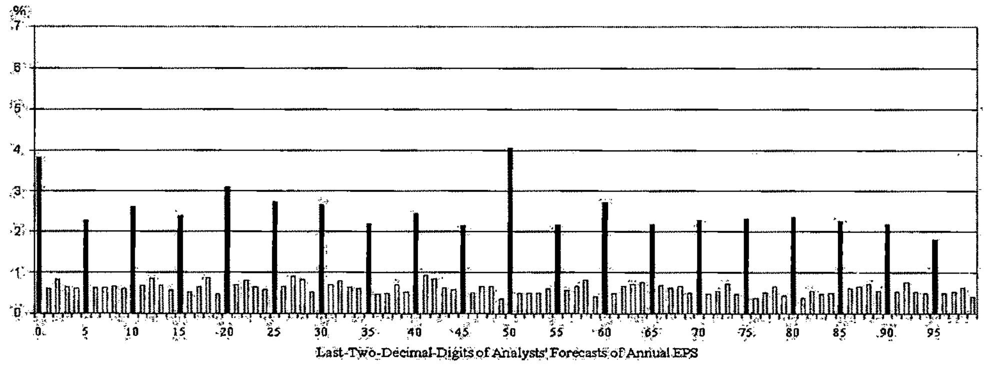
Panel A: Relative Frequencies of Last Two Digits of Management Point Forecasts of Annual EPS



Panel B: Relative Frequencies of Last Two Digits of Actual Annual EPS



Panel C: Relative Frequencies of Last Two Digits of Analysts' Forecasts of Annual EPS



Panel A shows the relative frequency of the last two digits of management's point forecasts of annual EPS collected by First Call (forecast type = "A," "F," and "Z") issued prior to the fiscal year-end for the period 1996–2004. Relative frequency is the number of EPS forecasts ending in each last-two-digit interval divided by the total number of EPS forecasts. Panel B shows the relative frequency of the last two digits of actual EPS for the sample in Panels A and C. Panel C shows the relative frequency of the last two digits of I/B/E/S analysts' forecasts of EPS issued within the [−90, −1] day window before the management forecast in Panel A.

consistency with the management forecast data). Comparison of these panels reveals that managers' forecasts heap at nickel intervals: Sharp increases in the frequency of management forecasts occur at every nickel interval, whereas we observe no obvious tendency for actual EPS to end in nickel intervals.

Table 2 provides more formal evidence. Panel A shows that the proportion of management forecasts significantly exceeds the proportion of subsequent actual EPS ending in even "dollar," "half-dollar," "dime," and "nickel" intervals ($z \geq 7.27$).[9] The right-most column shows that 46.98 percent of the management forecasts heap at one of the 20 nickel intervals, whereas subsequent actual EPS heap at nickel intervals only 20.12 percent of the time (z for the difference = 24.38).[10] Panel A further shows that managers' "dollar," "half-dollar," "dime," and "nickel" forecasts are 5.63, 5.00, 2.62, and 1.56 times more likely than are actual "dollar," "half-dollar," "dime," and "nickel" EPS. Evidence that the intensity of managers' heaping descends from the dollar, to half-dollar, to dime, to nickel intervals is consistent with Turner's (1958) theory that heaping is most extreme for the largest divisors.[11]

Figure 1 also shows that heaping in *management* forecasts (Panel A) is strikingly similar to heaping in *analysts'* forecasts issued within 90 days before the management forecast (Panel C). Panel B of Table 2 shows that although managers are slightly less likely than analysts to heap at dime and nickel intervals ($z \geq 1.96$), managers are *more* likely than analysts to heap at dollar and half-dollar intervals. Across all 20 nickel intervals, managers are slightly less likely to heap than analysts ($z = 5.04$), rejecting H1. But the magnitude of the difference is surprisingly small—46.98 percent of managers' forecasts heap versus 50.62 percent for analysts' forecasts.[12] Because managers exhibit similar heaping even though they have more private information about upcoming earnings, and even though they are forecasting over shorter horizons, factors beyond the existing theory of heaping (i.e., factors beyond uncertainty) must play a role in *managers'* heaping.

Properties of Managers' Nickel Forecasts

Table 3 provides evidence on the accuracy and bias in managers' forecasts relative to *ex post* actual EPS. Our measure of bias is the mean signed error in management's forecast (MFE)—the forecast minus actual EPS, deflated by the closing price one day prior to the management forecast date (e.g., McNichols 1989; Ajinkya et al. 2005). Thus, a positive value indicates the forecast was optimistic relative to *ex post* actual earnings. The mean absolute forecast error, |MFE|, measures (the inverse of) forecast accuracy.

Panel A of Table 3 shows that managers' nickel forecasts are less accurate than their non-nickel forecasts. Specifically, $|MFE|$ is higher for nickel forecasts than for non-nickel

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[9] The dollar interval ends in an even dollar amount such as –\$1.00, \$0.00, \$1.00, and so forth. The half-dollar interval ends in \$0.50; and the dime intervals end in \$0.10, \$0.20, \$0.30, \$0.40, \$0.60, \$0.70, \$0.80, and \$0.90. The nickel intervals end in \$0.05, \$0.15, \$0.25, \$0.35, \$0.45, \$0.55, \$0.65, \$0.75, \$0.85, and \$0.95. The right-most column of "All Nickel Intervals" includes all of the 20 intervals.

[10] Herrmann and Thomas (2005, 805) point out that Benford's law (Benford 1938) suggests that if total earnings are distributed randomly, then the probability that the pennies digit of EPS equals a 0 or 5 is very slightly higher than 20 percent: for EPS between \$1.00 and \$10.00, the pennies digit equals a 0 or 5 20.16 percent of the time.

[11] Heaping remains pervasive throughout the year. Even in the last three months of the fiscal year, 46 percent of newly issued management forecasts heap at nickel intervals.

[12] In additional analyses, we find that managers' propensity to issue EPS forecasts that heap at nickel intervals remains fairly constant across the 1996–2000 period (45.16 percent heap at nickel intervals) and the 2001–2004 period (47.59 percent heap at nickel intervals). In contrast, Herrmann and Thomas (2005) report that the propensity of *analysts'* forecasts to heap at nickel intervals decreased over their (earlier) sample period (1985–2001).

TABLE 2
Comparison of the Incidence of Nickel EPS in Management's Point Forecasts of Annual EPS, Actual EPS, and Analysts' Forecasts

Panel A: Percentages of Management Forecasts Relative to Actual EPS Heaped at Nickel Intervals[a]

	Dollar	Half-Dollar	Dime	Nickel	All Nickel Intervals
Number of management forecasts	197	155	639	543	1,534
As % of all management forecasts	6.03	4.74	19.57	16.63	46.98
% Actual EPS in nickel intervals	1.07	0.95	7.47	10.63	20.12
Test of difference in % (z-statistic)	(11.13)***	(9.42)***	(14.75)***	(7.27)***	(24.38)***
% of MF ÷ % Actual EPS	5.63	5.00	2.62	1.56	2.33

Panel B: Percentages of Management Forecasts Relative to Analysts' Forecasts Heaped at Nickel Intervals (H1)[b]

	Dollar	Half-Dollar	Dime	Nickel	All Nickel Intervals
Number of analysts' forecasts	896	952	4,771	5,265	11,884
As % of all analysts' forecasts	3.82	4.06	20.32	22.43	50.62
% of MF in nickel intervals (from Panel A)	6.03	4.74	19.57	16.63	46.98
Test of difference in % (z-statistic)	(−5.39)***	(−1.35)	(1.96)**	(7.14)***	(5.04)***
% of MF ÷ % AF	1.58	1.17	0.96	0.74	0.93

*, **, *** Indicate two-tailed significance at 10 percent, 5 percent, and 1 percent, respectively.

[a] Panel A compares the percentage of management's point forecasts of annual EPS to the percentage of subsequent actually reported EPS in: (1) dollar, (2) half-dollar, (3) dime, (4) nickel, and (5) all nickel intervals. The dollar interval ends in an even dollar amount; the half-dollar interval ends in $0.50; and the dime intervals end in $0.10, $0.20, $0.30, $0.40, $0.60, $0.70, $0.80, and $0.90. The nickel intervals end in $0.05, $0.15, $0.25, $0.35, $0.45, $0.55, $0.65, $0.75, $0.85, and $0.95. The right-most column of "All Nickel Intervals" includes all 20 intervals. z-statistics compare the difference between the percentage of management's forecasts and the percentage of actual EPS in each interval.

[b] Panel B compares the percentage of analysts' forecasts to the percentage of management forecasts in: (1) dollar, (2) half-dollar, (3) dime, (4) nickel, and (5) all nickel intervals. Analysts' forecasts include the most recent forecast each individual analyst issued within the [−90, −1] day window prior to management's forecast. z-statistics compare the difference between the percentage of analysts' forecasts and the percentage of management's forecasts in each interval.

forecasts (t = 8.15), supporting H2a.[13] The absolute forecast error ($|MFE|$) declines mon-

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[13] In all of our analyses, we winsorize continuous variables at 1 percent and 99 percent of their respective distributions.

TABLE 3
Accuracy and Bias in Management's Nickel Forecasts of Annual EPS (H2)

Panel A: Management Forecast Population (n = 3,265)[a]

	Dollar	Half-Dollar	Dime	Nickel	All Nickel	Non-Nickel	Nickel − Non-Nickel
n	197	155	639	543	1,534	1,731	
|*MFE*| (H2a)	0.027	0.019	0.015	0.012	0.016	0.009	0.007
(t-statistic)							(8.15)***
MFE (H2b)	0.019	0.012	0.008	0.007	0.009	0.005	0.004
(t-statistic)	(5.57)***	(3.57)***	(6.77)***	(6.07)***	(11.13)***	(10.60)***	(4.55)***

Panel B: Management Forecast Sample for Testing H3 and H4 (n = 1,510)[b]

	Dollar	Half-Dollar	Dime	Nickel	All Nickel	Non-Nickel	Nickel − Non-Nickel
n	87	77	295	229	688	822	
|*MFE*| (H2a)	0.029	0.014	0.011	0.010	0.014	0.006	0.007
(t-statistic)							(6.91)***
MFE (H2b)	0.013	0.010	0.007	0.006	0.008	0.004	0.005
(t-statistic)	(3.27)***	(2.99)***	(5.48)***	(3.51)***	(7.19)***	(6.51)***	(4.08)***

(*continued on next page*)

TABLE 3 (continued)

Panel C: Absolute Error in Debiased Management Forecasts (n = 1,510)[c]

	Differences in Mean			Differences in Median		
	Nickel	Non-Nickel	Nickel − Non-Nickel	Nickel	Non-Nickel	Nickel − Non-Nickel
Absolute forecast error in debiased forecasts	0.011	0.005	0.006	0.008	0.004	0.004
	t-test (two-tailed p-value): (5.48)***			Wilcoxon rank test (two-tailed p-value): (4.78)***		

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*, **, *** Indicate two-tailed significance at 10 percent, 5 percent, and 1 percent, respectively.

[a] Panel A shows for the population of management's point forecasts of annual EPS the mean absolute error and the mean signed error. *MFE* is the mean signed error in the management forecast, defined as the management forecast minus actual EPS, deflated by the closing price one day prior to the management forecast date. *MFE* is winsorized at the top and bottom 1 percent of its distribution.

[b] Panel B shows the mean absolute error and the mean signed error for the set of management forecasts with all the data necessary for testing H3 and H4.

[c] Panel C shows the mean and median values of the absolute forecast error remaining after debiasing the management forecasts, measured as the absolute value of the difference between the error in the management forecast and the predicted bias in the forecast calculated from Rogers and Stocken's (2005) model, where the difference is scaled by the closing price on day prior to the management forecast.

otonically as the granularity of the forecast decreases from a dollar, to a half-dollar, to a dime, and to a nickel, similar to the pattern that Herrmann and Thomas (2005) document for *analysts'* forecasts.

The mean signed forecast error (*MFE*) reported in Panel A shows that managers' nickel forecasts are optimistic relative to *ex post* actual EPS ($t = 11.13$), and this result also holds for each nickel interval ($t \geq 3.57$). Like forecast error, optimism declines as the granularity of the forecast decreases from a dollar, to a half-dollar, to a dime, and to a nickel. Consistent with H2b, forecast optimism (*MFE*) is almost twice as high for nickel forecasts as for non-nickel forecasts ($t = 4.55$). Evidence that managers' nickel forecasts are more optimistically biased than their non-nickel forecasts is important because it suggests that nickel forecasts are not simply benign heuristic responses to uncertainty (in which case nickel forecasts would be less accurate but unbiased). Panel B shows these inferences hold in the subsample with data for our determinants (H3) and consequences (H4) tests, so that subsample is representative of the population.

We next explore whether the lower accuracy of managers' nickel forecasts is simply the result of greater bias in these forecasts. We debias the forecasts by subtracting predicted bias from the management forecast. We measure predicted bias as the fitted value from the Rogers and Stocken (2005, Equation (1)) model that estimates the predictable portion of managers' *ex post* forecast errors based on managers' *ex ante* economic incentives to bias their forecasts (e.g., litigation risk, managers' upcoming sale or purchase of stock and options, the firm's financial health, and the general industry environment), and the likelihood that users will detect any intentional bias in the forecast. Panel C of Table 3 shows that even after adjusting for the greater predicted bias in nickel forecasts, managers' nickel forecasts are still less accurate than their non-nickel forecasts ($p \leq 0.01$),[14] which suggests that managers' incentives to bias provide a partial, but not complete, explanation of their propensity to issue nickel forecasts.

V. DETERMINANTS AND CONSEQUENCES OF MANAGERS' NICKEL FORECASTS

Determinants of Managers' Nickel Forecasts—Research Method

To test our hypotheses that uncertainty, concern for proprietary information, and economic incentives to issue optimistic forecasts are associated with heaping in managers' earnings forecasts, we estimate the following probit model:

$$\begin{aligned}\text{Prob}(\mathit{Nickel} = 1) = {} & \alpha_1 \mathit{Uncertainty} + \alpha_2 \mathit{PIC} + \alpha_3 \mathit{Pred\_Bias} \\ & + \alpha_4 \mathit{Unpred\_Bias} + \alpha_5 |\mathit{Actual\_EPS}| \\ & + \Sigma_i \alpha_i \mathit{Year}_i + \varepsilon_1 \qquad (1)\end{aligned}$$

where:

Nickel = 1 if management's point forecast of annual EPS ends in a nickel interval, and 0 otherwise;

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[14] The modest correlations between predicted bias and *ex post* errors in management forecasts (Spearman correlation = 0.25) confirms that these are two distinct, albeit related, constructs.

Uncertainty = constructed from a factor analysis of seven factors, following Rogers and Stocken (2005): the standard deviation of I/B/E/S analysts' forecasts immediately prior to the management forecast date; the standard deviation of I/B/E/S consensus forecast errors for the prior three to five years; the standard deviation of daily stock returns over 120 trading days prior to the management forecast; an indicator variable representing whether the firm reported a loss for the quarter prior to the management forecast; an indicator variable representing whether management is forecasting a loss for the fiscal year; the TAQ bid-ask spread divided by the quote midpoint, averaged over the prior 20 trading days; and the natural log of market value at the beginning of the fiscal year;[15,16]

PIC = a proxy for proprietary information costs, explained below;

Pred_Bias = predicted bias in the management forecast, measured as the fitted value of Rogers and Stocken's (2005, Equation (1)) model of bias in managers' forecasts;

Unpred_Bias = unpredicted bias, measured as the residual from Rogers and Stocken's (2005) model of the bias in managers' forecasts;

|*Actual_EPS*| = absolute value of actual earnings per share; and

Year = year fixed effect.

Figure 1 shows that heaping is most pronounced at the dollar and half-dollar intervals, and less pronounced at the dime and nickel intervals, so we also estimate an ordered probit counterpart to Equation (1): the dependent variable *Nickel* equals 2 if the forecast heaps at a dollar/half-dollar interval, 1 if the forecast heaps at a dime/nickel interval, and 0 for non-nickel forecasts.

PIC captures two elements of proprietary information costs: industry concentration and the firm's expenditures on research and development. Bamber and Cheon (1998) note that markets become concentrated when some firms obtain a strategic competitive advantage (e.g., Demsetz 1973; Liebeler 1978), and that competitors are more likely to change their actions in response to a firm's action (in this case a disclosure) when the product-market is more concentrated (Shepherd 1985).[17] As a result, revelation of information is more

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[15] We follow the method outlined in Rogers and Stocken (2005) with one minor deviation. They include range as well as point management forecasts, so their measure of uncertainty includes the width of range forecasts. Our sample includes only point forecasts, where *Width* is zero. A factor analysis of the remaining six variables is insufficiently specified because the eigenvalue for the primary factor is less than 1. Replacing *Width* with *Size*, the natural log of the firm's market value, yields a better-specified *Uncertainty* factor with an eigenvalue over 1. We chose *Size* as our replacement for *Width* because: (1) size is strongly correlated with the width of range forecasts reported by First Call during our sample period ($\rho = -0.434$), so it preserves the spirit of *Width*, and (2) a large body of research confirms there is less uncertainty about larger firms' future performance because their earnings streams are more persistent and less volatile (e.g., Whittington 1971, 67–71; Scherer 1980, 148–149; Lev 1983; Baber et al. 1998; Ali et al. 2007). Excluding *Size* from the measure of *Uncertainty* does not affect our inferences.

[16] We label this construct *Uncertainty*, because it captures uncertainty about earnings. (Reasoning that when there is more uncertainty about earnings, it is more difficult for users to evaluate the truthfulness with which the manager's forecast reveals his private information, Rogers and Stocken [2005] refer to this construct as *Difficulty*.)

[17] Similarly, Butler et al. (2007, 194) argue that "proprietary information costs are higher for firms in concentrated industries ... [which are] therefore more likely to protect economic rents" by disclosing less. For example, in Dontoh's (1989) model, firms adjust their production schedules in response to competitors' outcomes. Earlier information about competitors' outcomes (such as earnings forecasts) can allow firms to adjust their production schedules more rapidly, improving their positions at the expense of the forecasting competitor.

likely to dissipate the firm's competitive advantage in concentrated markets. The second element of our proprietary information cost measure follows the King et al. (1990) recommendation to use spending on research and development as an empirical proxy for proprietary information costs. Proprietary information costs are expected to increase with both industry concentration and research and development expenditures, so our measure combines these two elements:

$$PIC = Industry\_Concentration + R\&D \tag{2}$$

where:

Industry_Concentration = 1 if the Herfindahl index of the industry in which the firm operates (defined as the sum of squared market shares of all firms in the forecasting firm's six digit Global Industry Classification Standard)[18] is above the median of our sample, 0 otherwise; and

R&D = 1 if the firm's ratio of industry-adjusted research and development expenditure deflated by assets, measured at the end of the prior year, is above the median value of the sample, 0 otherwise.

The above- versus below-the-median bifurcation of *Industry_Concentration* and *R&D* allows us to incorporate these two variables of different scales into a single parsimonious measure of proprietary information costs. However, we obtain similar inferences when we specify the probit with separate (continuous) variables for industry concentration and R&D expenditures.

Our model also controls for *Unpred_Bias*, the unpredictable portion of the bias in managers' forecasts relative to *ex post* actual EPS. If our measure of predicted bias captures economic determinants of managers' propensity to issue biased forecasts, then *Unpred_Bias* will not differ across nickel and non-nickel forecasts (i.e., $\alpha_4 = 0$). We control for absolute EPS because managers' forecasts may be more likely to heap when EPS is larger. We also include year fixed effects to control for over-time changes in managers' disclosures (e.g., Bailey et al. 2003).[19]

Determinants of Managers' Nickel Forecasts—Results

Panel A of Table 4 presents descriptive statistics for the variables in our probit analysis. Mean *Pred_Bias* is positive (t = 22.12), indicating managers' incentives on average lead to predictably optimistic forecasts of annual earnings. Panel B reports correlations among the variables in this analysis, with Pearson correlations above the diagonal, and Spearman

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[18] Bhojraj et al. (2003) show that the GICS industry classification generates more homogenous industry groups than SIC codes or the Fama and French (1997) algorithm. Our results are unaffected if we instead use four-digit SIC codes. We also obtain similar results when we limit our sample to manufacturing firms and use the Herfindahl index provided in the U.S. Census Bureau's *Census of Manufactures* publications (Ali et al. 2009).

[19] Adding controls for the following does not affect our inferences, and the estimated coefficients for these additional control variables are not significant: an indicator variable that equals 1 if the absolute magnitude of management's EPS forecast is less than $0.95; the number of analysts following the firm; institutional ownership; and strength of corporate governance (the Gompers et al. [2003] GSCORE). Our inferences also continue to hold in a subsample where the effect of heaping is most material—where EPS is close to zero. When we rerun our analysis on the decile of management forecasts with the smallest absolute EPS, we find that uncertainty, predicted bias, and proprietary information costs are all significant at better than the 3 percent level.

TABLE 4
Descriptive Statistics for Determinants of Managers' Nickel Forecasts

Panel A: Descriptive Statistics[a]

	Mean	Std. Dev.	10%	Q1	Median	Q3	90%
Uncertainty	0.042	0.994	−0.989	−0.604	−0.142	0.486	1.252
PIC	1.000	0.662	0.000	0.000	1.000	1.000	2.000
Pred_Bias	0.005	0.009	−0.005	−0.001	0.004	0.010	0.016
Unpred_Bias	0.000	0.249	−0.016	−0.008	−0.001	0.006	0.015
|*Actual_EPS*|	1.726	1.204	0.51	0.93	1.43	2.31	3.11

Panel B: Correlations[b]

	Nickel	*Uncertainty*	*PIC*	*Pred_Bias*	*Unpred_Bias*	|*Actual_EPS*|
Nickel	1	0.077	0.071	0.093	0.010	0.121
		(0.00)	(0.03)	(0.00)	(0.71)	(0.00)
Uncertainty	0.059	1	0.077	0.444	0.000	−0.257
	(0.02)		(0.03)	(0.00)	(1.00)	(0.00)
PIC	0.071	0.082	1	0.013	0.003	−0.012
	(0.03)	(0.01)		(0.52)	(0.91)	(0.63)
Pred_Bias	0.075	0.410	0.018	1	0.000	−0.105
	(0.00)	(0.00)	(0.45)		(1.00)	(0.00)
Unpred_Bias	0.020	−0.101	0.011	−0.276	1	−0.118
	(0.43)	(0.00)	(0.64)	(0.00)		(0.00)
|*Actual_EPS*|	0.087	−0.400	−0.032	−0.178	−0.123	1
	(0.00)	(0.00)	(0.22)	(0.00)	(0.00)	

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[a] Panel A presents descriptive statistics for the determinants of managers' nickel forecasts. *Uncertainty* is constructed from a factor analysis of seven factors, following Rogers and Stocken (2005): the standard deviation in I/B/E/S analysts' forecasts immediately prior to the management forecast date; the standard deviation of I/B/E/S consensus forecast errors for the prior three to five years; the standard deviation of daily stock returns over 120 trading days prior to the management forecast; an indicator variable representing whether the firm reported a loss for the quarter prior to the management forecast; an indicator variable representing whether management is forecasting a loss for the fiscal year; the TAQ bid-ask spread divided by the quote midpoint, averaged over the prior 20 trading days; and the natural logarithm of market value at the beginning of the fiscal year. *PIC* = Industry_Concentration + R&D, where: Industry_Concentration = 1 if the Herfindahl index of the industry in which the firm operates (defined as the sum of squared market shares of all firms in the forecasting firm's six-digit Global Industry Classification Standard) is above the median value of the sample, 0 otherwise; and R&D = 1 if the firm's industry-adjusted ratio of research and development expenditure deflated by assets, measured at the end of the prior year, is above the median value of the sample, 0 otherwise. *Pred_Bias* is the fitted value of the Rogers and Stocken (2005) regression model that estimates managers' forecast bias. *Unpred_Bias* is the residual of the Rogers and Stocken (2005) regression model that estimates managers' forecast bias. |*Actual_EPS*| is the absolute value of actual EPS.

[b] Panel B presents Pearson (above the diagonal) and Spearman (below the diagonal) correlations and two-tailed p-values. *Nickel* is 1 if managers' forecast ends in a nickel interval, and 0 otherwise. In this and all subsequent analyses, all continuous variables are winsorized at the top and bottom 1 percent of their respective distributions.

correlations below. These correlations provide univariate evidence consistent with our hypotheses. Specifically, management forecasts ending in nickel intervals are associated with higher uncertainty about earnings (*Uncertainty*), higher proprietary information costs (*PIC*), and greater predicted bias (*Pred_Bias*), all at $p \leq 0.03$, consistent with H3a, H3b, and H3c.

Table 5 presents the probit results. We expect uncertainty, proprietary information costs, and predicted bias to play stronger roles in explaining managers' longer-horizon forecasts.

TABLE 5
Determinants of Managers' Nickel Forecasts (H3)

$$\text{Prob}(\textit{Nickel}) = \alpha_1 \textit{Uncertainty} + \alpha_2 \textit{PIC} + \alpha_3 \textit{Pred\_Bias} + \alpha_4 \textit{Unpred\_Bias} + \alpha_5 |\textit{Actual\_EPS}| + \Sigma_i \alpha_i \textit{Year}_i + \varepsilon_1$$

		Probit Model[a]			Ordered Probit[b]
		Nickel = 1 for All Nickel Nickel = 0 for Non-Nickel			Nickel = 2 for Half-Dollar and Dollar Nickel = 1 for Nickel and Dime Nickel = 0 for Non-Nickel
Variable	**Predicted Sign**	**Full Sample**	**First Forecast**	**Revised Forecast**	**Full Sample**
Constant	?	−0.471* (−1.96)	−1.076** (−2.46)	−0.251 (−1.19)	NA
Uncertainty	+ (H3a)	0.163*** [*0.07*] (3.16)	0.297*** [*0.13*] (2.86)	0.106*** [*0.04*] (1.99)	0.138*** [*0.03*] (3.35)
PIC	+ (H3b)	0.129*** [*0.05*] (2.13)	0.189*** [*0.08*] (2.03)	0.117*** [*0.04*] (2.00)	0.119*** [*0.02*] (1.99)
Pred_Bias	+ (H3c)	11.100*** [*0.05*] (2.18)	26.434*** [*0.11*] (2.16)	9.551** [*0.03*] (1.82)	13.346*** [*0.03*] (3.13)
Unpred_Bias	?	0.705 [*0.00*] (0.44)	0.316 [*0.00*] (0.21)	1.807 [*0.01*] (1.00)	0.840 [*0.00*] (0.53)
|*Actual_EPS*|	+	0.176*** [*0.09*] (3.54)	0.269*** [*0.14*] (3.51)	0.121*** [*0.06*] (2.74)	0.200*** [*0.05*] (4.25)
Year Fixed Effects		yes	yes	yes	yes
n		1,510	894	616	1,510
Pseudo R^2 (%)		8.78	12.80	5.37	NA

(*continued on next page*)

TABLE 5 (continued)

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*, **, *** Indicate one-tailed significance for signed predictions and two-tailed significance for unsigned predictions at 10 percent, 5 percent, and 1 percent, respectively.

[a] This table presents the estimated coefficients, marginal effects in [*italics*], and z-statistics in (parentheses) for the probit model testing the determinants of management nickel forecasts for full sample of management point forecasts of annual EPS, and separately for: (1) the first management forecast, and (2) revised forecasts. In the primary (dichotomous) probit model, the dependent variable is *Nickel*, which equals 1 if the manager's forecast ends in a nickel interval, and 0 otherwise. Other variables are defined in Table 4. For continuous variables, the marginal effect is the effect of a one standard deviation change in the explanatory variable on the probability the management forecast ends in a nickel interval (holding other variables at their mean values). For discrete variables (e.g., *PIC*), the marginal effect is the effect of a one unit change in the explanatory variable on the probability that the management forecast ends in a nickel interval. Z-statistics are based on Murphy and Topel (1985) robust standard errors adjusted for clustering by firm. Following Veall and Zimmermann's (1996) recommendation, we report McKelvey and Zavoina's (1975) pseudo R^2.

[b] In the ordered probit model, the dependent variable takes on the value 2 if the management forecast ends in a half-dollar or dollar interval, 1 if the forecast ends in a nickel or dime interval, and 0 if it ends in a non-nickel interval. Because the proportional odds assumption (i.e., the parallel regression assumption) is met, the average marginal effects of moving from the non-nickel to the nickel/dime category (i.e., from 0 to 1 in the dependent variable) are not significantly different from the average marginal effects of moving from the nickel/dime to the half-dollar/dollar category (i.e., from 1 to 2 in the dependent variable). This is why the average marginal effect of moving up one category in the ordered probit equals half the total marginal effect reported in column 1.

Longer horizon forecasts are subject to more uncertainty. Longer horizons extend the period over which the firm potentially incurs proprietary information costs if the disclosure prompts competitors to change their actions in a way that allows them to compete more effectively against the firm. Longer horizons also extend the period over which managers benefit from opportunistically biased forecasts (indeed, Rogers and Stocken's [2005] measure of predicted bias was developed specifically for the longest-horizon forecasts—managers' first forecasts of annual earnings). Thus, in addition to estimating the model over the entire sample, we also estimate the probit model after partitioning managers' *initial* forecasts of annual earnings (average horizon is 203 days) *vis-à-vis revised* forecasts (average horizon is 164 days).[20]

We focus first on the probit model reported in the left side of Table 5. Managers' propensity to issue nickel EPS forecasts increases with uncertainty about earnings: the *Uncertainty* coefficient is positive for the full sample as well as for the initial forecasts and the revised forecasts ($p \leq 0.01$), consistent with H3a. For the sample as a whole, the marginal effect indicates that a one standard deviation increase in *Uncertainty* leads to a seven percentage point increase in the probability that the manager's forecast heaps at a nickel interval. This effect is material in relation to the unconditional probability that the manager's forecast heaps (47 percent, as shown in Panel B of Table 2).

As expected, uncertainty plays a more significant role in the longer-horizon initial forecasts than in the revised forecasts. A one standard deviation increase in *Uncertainty* is associated with a 13 percentage point increase in the probability that the manager's initial forecast heaps at a nickel interval, but a four percentage point increase for revised forecasts. In untabulated analyses, we include the entire sample in a single probit model, using indicator variables to distinguish incremental slope effects for the initial forecasts. This analysis reveals that the coefficient on *Uncertainty* is higher for the initial forecasts than for revised forecasts ($p \leq 0.10$).

Table 5 reports a positive coefficient for our proxy for proprietary information costs (*PIC*), for the full sample as well as for the initial forecasts and the revised forecasts ($p \leq 0.01$), supporting H3b. Among managers who issue forecasts of EPS, those whose firms' have higher proprietary information costs are more likely to issue the noisier, heaped forecasts. Proprietary information costs play a larger role in explaining heaping in the longer-horizon initial forecasts than in the shorter-horizon revised forecasts: A one-unit increase in *PIC* is associated with an eight percentage point increase in the probability that a manager's initial forecast heaps at a nickel interval, and a four percentage point increase in the probability that a revised forecast heaps.

We find a positive coefficient for *Pred_Bias* for the sample as a whole as well as for the initial forecasts and the revised forecasts ($p \leq 0.05$), supporting H3c. That is, managers' forecasts are more likely to heap at nickel intervals when managers have greater incentives to bias their forecasts optimistically. Predicted bias plays a more significant role in explaining heaping in the longer-horizon initial forecasts. A one standard deviation increase in predicted bias is associated with an 11 percentage point increase in the probability that initial forecasts heap, but a three percentage point increase for revised forecasts. Untabulated analyses confirm that the coefficient on *Pred_Bias* is higher in the set of initial forecasts ($p \leq 0.10$).

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[20] Inferences from our probit and subsequent OLS regressions are all robust after deleting observations with studentized residuals over 2.0.

In contrast, the estimated coefficients for *Unpred_Bias* are insignificant ($p \geq 0.43$). Finding that predicted bias (but not unpredicted bias) is associated with managers' propensity to issue nickel forecasts suggests that our proxy for predicted bias effectively captures managers' key incentives to issue biased forecasts. Evidence linking managers' propensity to issue (primarily optimistic) nickel forecasts to their economic incentives to optimistically bias their forecasts is important because it suggests the bias in managers' nickel forecasts stems from managers' economic incentives (and is not just the result of unconditional exuberance).

The right-most column of Table 5 shows that the inferences continue to hold in the ordered probit.[21] The greater the uncertainty, the firm's proprietary information costs, and the predicted bias, the more likely the manager's forecast heaps at increasingly coarse intervals.[22]

Finally, in untabulated sensitivity tests, we find that adding a control for forecast horizon does not affect our inferences, because uncertainty, proprietary information costs, and predicted bias all remain significant ($p \leq 0.01$). When we partition the sample at the median forecast horizon, however, uncertainty, proprietary information costs, and predicted bias are significant in both subsamples ($p \leq 0.10$), but the effects are stronger in the longer-horizon subsample than in the shorter-horizon subsample, suggesting horizon moderates the uncertainty, proprietary information cost, and predicted bias effects, rather than acting as a direct main effect on heaping.

Consequences of Managers' Nickel Forecasts—Does Heaping in Management Forecasts Spur Analysts' Heaping?

To test H4a's prediction that part of the heaping in analysts' forecasts observed in Herrmann and Thomas (2005) is spurred by heaping in management forecasts, we investigate revisions in analysts' forecasts immediately following managers' forecasts. Panel A of Table 6 presents frequencies of analysts' nickel forecasts before and after: (1) managers' nickel forecasts, and (2) managers' non-nickel forecasts. We include analysts who issue a forecast within 90 days before the management forecast and then revise that forecast within five days after the management forecast. This reduces our sample from 1,510 to 1,038 management forecasts. If an analyst issues multiple forecasts within either the pre- or post-window, then we retain the one closest to the management forecast date. Requiring analysts to update their forecasts within five days after the management forecast ensures that these are active analysts following the firm on a timely basis. Supplementary analyses confirm that these are more active analysts: analysts who revise within five days after the management forecast issue more forecasts that are also more accurate; they work for larger brokers who on average provide more resources; and they cover fewer industries than other analysts following the same firm who do not promptly update ($p \leq 0.05$).

Table 6, Panel A shows that 486 of the 1,038 management forecasts (46.82 percent) heap at nickel intervals, similar to the proportion of heaping in the full population (46.98

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[21] A test of the proportional odds assumption (the parallel regression assumption) supports our use of ordered probit.

[22] The expectations adjustment hypothesis (Ajinkya and Gift 1984) suggests that managers issue forecasts to *correct* expectations, so managers should not be concerned with matching prior analysts' forecasts. Nonetheless, to ensure that managers are not heaping in response to analysts' forecasts, we add to our determinants model (Equation (1)) a variable to control for the proportion of analysts' forecasts heaped at nickel intervals *before* the management forecast. We also add a variable to control for whether the subsequent actual EPS ended in a nickel interval. Neither control variable is significant, and neither control affects our inferences. Thus, we find no evidence that heaping in analyst forecasts (or heaping in subsequent actual EPS) spurs heaping in management forecasts.

TABLE 6
Frequency of Analysts' Nickel Forecasts Before and After Managers' Nickel versus Non-Nickel Forecasts (H4a)[a]

Panel A: Full Sample

	Conditional on Management Nickel Forecast				Conditional on Management Non-Nickel Forecast			
	Actual EPS	Analysts' Forecasts			Actual EPS	Analysts' Forecasts		
		Prior	Revised	Diff1		Prior	Revised	Diff2
As % of all forecasts	20%	48% [22.8]***	61% [33.6]***	13% (10.40)***	19%	40% [16.5]***	29% [8.9]***	11% (8.96)***
n	486	2,109	2,109	2,109	552	2,356	2,356	2,356

Chi-square test on (Diff1 − Diff2) = 1.31 (p = 0.25)

Panel B: First Management Forecast

	Conditional on Management Nickel Forecast				Conditional on Management Non-Nickel Forecast			
	Actual EPS	Analysts' Forecasts			Actual EPS	Analysts' Forecasts		
		Prior	Revised	Diff1		Prior	Revised	Diff2
As % of all forecasts	23%	49% [15.9]***	61% [24.0]***	12% (7.97)***	21%	42% [11.8]***	32% [5.6]***	10% (7.10)***
n	314	1,113	1,113	1,113	322	1,101	1,101	1,101

Chi-square test on (Diff1 − Diff2) = 1.14 (p = 0.28)

Panel C: Revised Management Forecast

	Conditional on Management Nickel Forecast				Conditional on Management Non-Nickel Forecast			
	Actual EPS	Analysts' Forecasts			Actual EPS	Analysts' Forecasts		
		Prior	Revised	Diff1		Prior	Revised	Diff2
As % of all forecasts	17%	46% [14.7]***	63% [25.1]***	17% (9.81)***	17%	46% [15.3]***	27% [4.9]***	19% (7.82)***
n	172	996	996	996	230	1,255	1,255	1,255

Chi-square test on (Diff1 − Diff2) = 1.72 (p = 0.16)

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*, **, *** Indicate two-tailed significance at 10 percent, 5 percent, and 1 percent, respectively.

[a] This table presents the frequency of analysts' nickel forecasts. To be included in this analysis, an analyst must issue a forecast within [−90, −1] days before the management forecast and then revise that forecast within [+1, +5] days after the management forecast. Actual EPS is the percentage of actual EPS that end in a nickel interval. Prior is the percentage of analysts' forecasts ending in a nickel interval before managers' nickel (non-nickel) forecasts. Revised is the percentage of analysts' forecasts ending in a nickel interval after managers' nickel (non-nickel) forecasts. t-statistics in [brackets] indicate whether prior (revised) minus actual is significantly different from zero. t-statistics on Diff1 and Diff2 in (parentheses) indicate whether the revised minus the prior differs from zero. The Chi-square tests the null hypothesis that Diff1 (analysts' revision conditional on management nickel forecasts) = Diff2 (analysts' revision conditional on management non-nickel forecasts).

percent per Table 2). After managers issue nickel forecasts, the proportion of analysts issuing nickel forecasts increases by 13 percentage points, from 48 percent to 61 percent ($t = 10.40$). Thus, while Herrmann and Thomas (2005) conclude that more active analysts are less likely to heap, we find nickel management forecasts spur heaping even among active analysts, consistent with H4a. When managers issue non-nickel forecasts, analysts' nickel forecasts drop by 11 percentage points, from 40 percent to 29 percent ($t = 8.96$). Panels B and C show these inferences hold for both initial and revised management forecasts.

Even though managers' nickel forecasts are on average more optimistically biased and less accurate than their non-nickel forecasts (Table 3), Table 6 shows that a nickel management forecast is equally likely to spur analysts to revise to nickel forecasts as a non-nickel management forecast is to spur analysts to revise to non-nickel forecasts (the difference is not significant at conventional levels, $p \geq 0.16$). This raises the question whether analysts appreciate the lower accuracy of management forecasts heaped at nickel intervals.

Consequences of Managers' Nickel Forecasts—Do Analysts Adjust for Lower Accuracy?

To investigate whether analysts adjust for the lower accuracy of managers' nickel relative to non-nickel forecasts, we estimate the following model:

$$\begin{aligned} Revision = {} & \delta_0 + \delta_1 Good \times MF\_News + \delta_2 Bad \times MF\_News \\ & + \delta_3 Good \times MF\_News \times Nickel \\ & + \delta_4 Bad \times MF\_News \times Nickel + \Sigma_n \delta_n Controls + \varepsilon_3 \end{aligned} \quad (3)$$

where:

Revision = the average of analysts' forecasts issued within [+1, +5] days after the management forecast minus the average of analysts' forecasts issued within [−90, −1] days before the management forecast, deflated by the closing price one day prior to the management forecast date;
Good = 1 if *MF_News* is non-negative, and 0 otherwise;
Bad = 1 if *MF_News* is negative, and 0 otherwise; and
MF_News = news in the management forecast, defined as the management forecast minus the most recent I/B/E/S consensus analyst forecast, deflated by the closing price one day prior to the management forecast date.

Equation (3) controls for factors that moderate the response coefficient on management forecast news: an indicator variable if management forecasts a loss, and forecast horizon. We also control for the (signed) squared news in the management forecast to allow a nonlinear relation between earnings news and analysts' revisions. Finally, when managers forecast earnings on the same day they announce actual earnings of a prior period, we also control for the surprise in the earnings announcement. If analysts recognize that managers' nickel forecasts are less accurate, then H4b posits that analysts' revision per unit of nickel management forecast news will be smaller than the revision per unit of non-nickel news. Thus, H4b suggests $\delta_3 < 0$ and $\delta_4 < 0$.

Table 7 reports the results of estimating Model 3 on the full sample, the first forecasts, and the revised forecasts. The left side of the table presents results based on the dichotomous nickel/non-nickel specification, whereas the right-most columns present results based

TABLE 7
Analysts' Revisions in Response to Management Forecasts (H4b)[a]

Model 3: $Rev = \delta_0 + \delta_1 Good \times MF\_News + \delta_2 Bad \times MF\_News + \delta_3 Good \times MF\_News \times Nickel + \delta_4 Bad \times MF\_News \times Nickel + \Sigma_n \delta_n Controls + \varepsilon_3$

Model 4: $Rev = \delta_0 + \delta_1 Good \times MF\_News + \delta_2 Bad \times MF\_News + \delta_3 Good \times MF\_News \times Nickel + \delta_4 Bad \times MF\_News \times Nickel + \delta_5 Good \times MF\_News \times Pred\_Bias + \delta_6 Bad \times MF\_News \times Pred\_Bias + \Sigma_n \delta_n Controls + \varepsilon_4$

		Nickel = 1 for All Nickel Nickel = 0 for All Non-Nickel						**Nickel = 2 for Half-Dollar and Dollar Nickel = 1 for Nickel and Dime Nickel = 0 for Non-Nickel**	
		Model 3			**Model 4**			**Model 3**	**Model 4**
Independent Variables	**Predicted Sign**	**Full Sample**	**First Forecast**	**Revised Forecast**	**Full Sample**	**First Forecast**	**Revised Forecast**	**Full Sample**	
Constant	?	0.002	0.004	−0.009	−0.000	0.001	−0.008	0.002	−0.001
		(0.36)	(0.75)	(−1.18)	(−0.03)	(0.14)	(−1.00)	(0.34)	(−0.10)
Good × *MF_News*	+	1.345***	1.459***	1.014***	1.305***	1.529***	0.934***	1.285***	1.253***
		(6.05)	(6.68)	(2.20)	(5.48)	(5.45)	(2.09)	(5.81)	(5.28)
Bad × *MF_News*	+	0.418***	0.281*	0.552***	0.469***	0.398***	0.473***	0.408***	0.473***
		(2.83)	(1.51)	(2.80)	(3.36)	(2.18)	(2.58)	(2.82)	(3.47)
Good × *MF_News*	− (H4b)	−0.374***	−0.562***	−0.248*	−0.312***	−0.610***	0.002	−0.273***	−0.246***
× *Nickel*		(−2.29)	(−2.08)	(−1.28)	(−2.13)	(−2.25)	(0.01)	(−2.35)	(−2.37)
Bad × *MF_News*	− (H4b)	−0.082	−0.111	0.099	−0.063	−0.087	0.095	−0.074*	−0.072*
× *Nickel*		(−0.97)	(−1.19)	(0.56)	(−0.77)	(−0.98)	(0.53)	(−1.38)	(−1.38)
Good × *MF_News*	−				−16.248***	−22.059***	−26.429***		−16.568***
× *Pred_Bias*					(−2.53)	(−1.98)	(−4.39)		(−2.78)
Bad × *MF_News*	+				10.624*	10.093*	4.063		11.554**
× *Pred_Bias*					(1.61)	(1.46)	(0.31)		(1.76)

(*continued on next page*)

TABLE 7 (continued)

		Nickel = 1 for All Nickel Nickel = 0 for All Non-Nickel						Nickel = 2 for Half-Dollar and Dollar Nickel = 1 for Nickel and Dime Nickel = 0 for Non-Nickel	
		Model 3			Model 4			Model 3	Model 4
Control Variables	**Predicted Sign**	**Full Sample**	**First Forecast**	**Revised Forecast**	**Full Sample**	**First Forecast**	**Revised Forecast**	**Full Sample**	
MF_News × *PredLoss*	−	−0.038	−0.690***	0.287	−0.128	−0.792***	0.061	0.034	−0.062
		(−0.19)	(−2.39)	(1.69)	(−0.70)	(−4.02)	(0.53)	(0.18)	(−0.39)
MF_News × *Horizon*	−	−0.000	0.000	−0.000	0.000	0.000	0.000	−0.000	0.000
		(−0.64)	(0.36)	(−0.08)	(0.06)	(0.88)	(0.26)	(−0.58)	(0.24)
MF_News × |*MF_News*|	−	−1.080	−1.079	−0.544	−0.886	−1.549*	0.128	−0.913	−0.702
		(−1.18)	(−1.09)	(−0.29)	(−0.95)	(−1.29)	(0.07)	(−1.00)	(−0.75)
Surprise	+	−0.000	0.000	−0.005	−0.001	−0.001	−0.005	−0.000	−0.001
		(−0.05)	(0.08)	(−0.58)	(−0.28)	(−0.15)	(−0.51)	(−0.04)	(−0.27)
n		1,510	894	616	1,510	894	616	1,510	1,510
R^2 (%)		31.41	26.57	46.69	32.29	27.47	49.25	31.46	32.52
F-test		6.16	5.45	0.32	4.92	5.97	0.14	7.43	7.39
(joint test of H4b)		p = 0.01	p = 0.02	p = 0.57	p = 0.03	p = 0.02	p = 0.71	p = 0.01	p = 0.01

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*, **, *** Indicate one-tailed significance for signed predictions and two-tailed significance for unsigned predictions at 10 percent, 5 percent, and 1 percent, respectively.

[a] This table presents the results of regressions explaining analysts' forecast revisions in response to managers' nickel and non-nickel forecasts. *Rev* is the average of analysts' forecasts issued within [+1, +5] days after the management forecast minus the average of analysts' forecasts issued within [−90, −1] days before the management forecast, deflated by the closing price one day prior to the management forecast date. *MF_News* is the management forecast minus the most recent I/B/E/S consensus analyst forecast, deflated by the closing price one day prior to the management forecast date. *Good* is 1 if *MF_News* is nonnegative, and 0 otherwise. *Bad* is 1 if *MF_News* is negative, and 0 otherwise. *Pred_Bias* is the fitted value of the Rogers and Stocken (2005) regression model that estimates managers' forecast bias. *PredLoss* is 1 if management forecasts a loss, and 0 otherwise. *Horizon* is the number of calendar days between the management forecast date and earnings announcement date. *Surprise* equals actual EPS minus the prevailing consensus analyst forecast deflated by closing price the day before the earnings announcement if managers issue a forecast of future annual earnings on the same day they announce actual earnings for a prior period. If the management forecast is *not* issued on the same day as an earnings announcement, then *Surprise* is 0. t-statistics are based on Murphy and Topel's (1985) robust standard errors adjusted for clustering by firm. The F-test at the bottom of the table examines whether the sum of the estimated δ_3 and δ_4 coefficients equals 0.

on the categorical specification where *Nickel* is coded as 2 if the forecast heaps at a dollar or half-dollar, 1 if the forecast heaps at a dime or nickel, and 0 if the forecast ends in a non-nickel interval.

Table 7 shows that for the full sample, the subsample of first forecasts, and the categorical specification, the coefficient for *Good* × *MF_News* × *Nickel*, δ_3, is negative ($p \leq 0.01$), suggesting that analysts discount managers' good news nickel forecasts. Although the coefficient for *Bad* × *MF_News* × *Nickel*, δ_4, is not significant (except marginally so in the categorical specification), the F-tests reported at the bottom of Table 7 support H4b by rejecting the null that the estimated coefficients for *Good* × *MF_News* × *Nickel* and *Bad* × *MF_News* × *Nickel* are jointly insignificant ($p \leq 0.02$) in the full sample, the subsample of first forecasts, and in the categorical specification. Thus, in contrast to Herrmann and Thomas' (2005) conclusion that *share prices* do not adjust for the lower accuracy of heaped *analysts'* forecasts, our results suggest analysts do adjust for the additional noise in heaped management forecasts—at least longer-horizon forecasts that convey good news. Again, this may be because analysts' forecasts reflect more sophisticated earnings expectations, and/or because revisions of analysts' forecasts are sharper measures of the effect of managers' EPS forecasts on expectations of the same EPS number.

Analysts may discount nickel management forecasts either because of the additional predictable bias in nickel forecasts, or because nickel forecasts are less accurate even after controlling for predictable bias. To provide further insight into this issue, Model 4 augments Model 3 by allowing the management forecast news response coefficient to depend on the predicted bias in the management forecast:

$$\begin{aligned} Revision = {} & \delta_0 + \delta_1 Good \times MF\_News + \delta_2 Bad \times MF\_News \\ & + \delta_3 Good \times MF\_News \times Nickel + \delta_4 Bad \times MF\_News \times Nickel \\ & + \delta_5 Good \times MF\_News \times Pred\_Bias \\ & + \delta_6 Bad \times MF\_News \times Pred\_Bias + \Sigma_n \delta_n Controls + \varepsilon_4. \end{aligned} \tag{4}$$

If analysts discount nickel management forecasts because they are less accurate even after adjusting for the predictable bias, then analysts' revision per unit of news should remain smaller in response to nickel than non-nickel management forecasts (i.e., $\delta_3 < 0$ and $\delta_4 < 0$). Model 4 also allows us to ascertain whether analysts appear to adjust for the predictable bias in management forecasts. If analysts' revision per unit of good news in the management forecast is attenuated proportionally to the portion of that good news that is the predictable bias, then $\delta_5 < 0$. Similarly, if analysts' revision per unit of bad news is exacerbated in proportion to the extent to which the bad news is understated as a result of the predictable bias in the forecast, then $\delta_6 > 0$.

Table 7 shows that the results of Model 4 are largely consistent with these expectations. Even after controlling for predicted bias, in the full sample and the initial forecasts, analysts' revision per unit of good news remains smaller in response to nickel than non-nickel management forecasts (i.e., $\delta_3 < 0$), consistent with H4b. Analysts also appear to adjust for predicted bias in forecasts. The coefficient for *Good* × *MF_News* × *Pred_Bias*, δ_5, is negative in all specifications ($p \geq 0.01$). That is, analysts' revision per unit of good news in the management forecast is attenuated proportionally to the component of the news that is the predictable bias. The coefficient for *Bad* × *MF_News* × *Pred_Bias*, δ_6, is significantly positive in the categorical specification ($p \leq 0.05$), and marginally significant in the full sample and in the initial forecasts ($p \leq 0.10$), providing some evidence that analysts' revision per unit of bad forecast news is exacerbated in proportion to the extent bad news is understated as a result of predictable bias. Evidence that analysts appear to make some adjustment for predicted bias in managers' bad news forecasts is interesting given Rogers and Stocken's (2005) conclusion that (after deleting outliers) *share prices* do not adjust for predicted bias in bad news management forecasts. This difference could arise because analysts' forecasts reflect more sophisticated earnings expectations, and/or because revisions in analysts' forecasts are sharper measures of the effect of management earnings forecasts on market participants' expectations of the same EPS number.

The overall pattern of results suggests that analysts are more likely to discount the news in (noisier) nickel forecasts if the forecast conveys goods news. This result is consistent with evidence that market participants tend to take bad news forecasts at face value, while eyeing good news forecasts more skeptically (e.g., Jennings 1987; Williams 1996; Hutton et al. 2003).

Supplementary examination of how management forecasts affect the *error* in analysts' revised forecasts similarly suggests analysts place less weight on managers' nickel forecasts. The first row of Table 8, Panel A reports the signed change in the error in the consensus analyst forecast in response to the management forecast, and the change in absolute error in the consensus forecast. Non-nickel management forecasts lead to a more pronounced decrease in analyst optimism ($t = 1.91$), and a more pronounced decrease in absolute forecast error ($t = 2.56$). We also examine the signed and absolute error in the (consensus) analyst forecasts issued within five days after the management forecast. The second row of Table 8 shows that analysts' forecasts remain more optimistic ($t = 2.41$) and less accurate ($t = 3.14$) after nickel management forecasts than after non-nickel forecasts. Panels B and C show these patterns hold for the first forecasts and revised forecasts. Thus, while analysts place less weight on managers' nickel forecasts, they do not fully adjust for the additional optimism.

VI. ADDITIONAL ANALYSES

Rogers and Stocken (2005, 1235) question whether heightened scrutiny of corporate financial disclosures (stemming from Reg FD and Sarbanes-Oxley) reduced managers' propensity to issue biased forecasts after their sample period ended in 2000. We find that neither accuracy ($|MFE|$) nor optimism (MFE) changed significantly from the 1996–2000 to the 2001–2004 period. Managers' nickel forecasts of annual EPS remain more optimistically biased than non-nickel forecasts after Reg FD ($p \leq 0.01$). Uncertainty, proprietary information costs, and predicted bias remain significant explanators of heaping in both periods ($p \leq 0.07$). We therefore conclude that increased scrutiny has not affected managers' propensity to issue optimistically biased nickel forecasts of annual EPS.

Managers' point forecasts of EPS provide the cleanest setting for testing our hypotheses, because measurement of bias and accuracy (relative to *ex post* actual EPS) is ambiguous for nonpoint forecasts. If managers issue a \$4.05–\$4.09 range forecast, and actual EPS is \$4.06, then the forecast is optimistically biased and inaccurate relative to the range's *midpoint* (the benchmark typically used in prior research), but the forecast is neither biased nor inaccurate in that actual EPS falls within the range of the forecast. For this reason, research on bias and accuracy commonly uses point forecasts (e.g., Ajinkya et al. 2005; Karamanou and Vafeas 2005).

To assess the generalizability of our inferences based on point forecasts, we perform supplementary analyses on range forecasts. We use the midpoint of the range as our "point estimate" to determine bias and accuracy, and code nickel forecasts as those where at least one endpoint ends in a nickel interval. The inferences are quite similar to those based on

TABLE 8
Signed and Absolute Errors in Consensus Analyst Forecasts Conditional on Managers' Nickel versus Non-Nickel Forecasts

Panel A: Full Sample

	Signed Analyst Forecast Error Conditional On			Absolute Analyst Forecast Error Conditional On		
	MF Nickel	MF Non-Nickel	Diff	MF Nickel	MF Non-Nickel	Diff
Change in error in consensus analyst forecast[a]	−0.141	−0.257	0.116 (1.91)*	−0.204	−0.338	0.134 (2.56)***
Analysts' forecast error after management forecast[b]	0.782	0.441	0.341 (2.41)**	1.270	0.836	0.431 (3.14)***

Panel B: First Forecast

	Signed Analyst Forecast Error Conditional On			Absolute Analyst Forecast Error Conditional On		
	MF Nickel	MF Non-Nickel	Diff	MF Nickel	MF Non-Nickel	Diff
Change in error in consensus analyst forecast	−0.192	−0.299	0.107 (1.74)*	−0.230	−0.368	0.139 (2.16)**
Analysts' forecast error after management forecast	0.877	0.501	0.377 (2.53)**	1.435	0.963	0.472 (2.72)***

Panel C: Revised Forecast

	Signed Analyst Forecast Error Conditional On			Absolute Analyst Forecast Error Conditional On		
	MF Nickel	MF Non-Nickel	Diff	MF Nickel	MF Non-Nickel	Diff
Change in error in consensus analyst forecast	−0.024	−0.212	0.188** (1.99)	−0.144	−0.306	0.162 (1.79)*
Analysts' forecast error after management forecast	0.564	0.377	0.187 (1.98)**	0.891	0.701	0.191 (2.24)**

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*, **, *** Indicate two-tailed significance at 10 percent, 5 percent, and 1 percent, respectively.

[a] The change in the error in the consensus analyst forecast is defined as error in the consensus of analysts' forecasts issued within [+1, +5] days after the management forecast, minus the error in the consensus of analysts' forecasts issued within [−90, −1] days before the management forecast, deflated by the closing price one day prior to the management forecast date (multiplied by 100).

[b] Analysts' forecast error after the management forecast is defined as the error in the consensus of analysts' forecast issued within [+1, +5] days after management forecast, deflated by the closing price one day prior to the management forecast date (multiplied by 100).

point forecasts: 64 percent of range forecasts heap at nickel intervals, and heaping increases with uncertainty, proprietary information costs, and predicted bias ($p \leq 0.01$). As for point forecasts, analysts further discount the news in (the noisier) nickel management forecasts, even after controlling for predicted bias. Thus, our inferences generalize beyond point forecasts.

We focus on management forecasts of annual earnings because we hypothesize that heaping stems from uncertainty, desire to attenuate proprietary information costs, and managerial opportunism, all of which play more significant roles in longer horizon choices.[23] Table 5 confirms that these determinants play a more significant role in explaining heaping in the longer-horizon initial forecasts of annual earnings than in the shorter-horizon revisions of forecasts.

To further assess the moderating role of forecast horizon, we conducted ancillary analyses of managers' forecasts of quarterly EPS, which have much shorter horizons (58 days) than the forecasts of annual earnings (187 days). Consistent with the conjecture (and ancillary results) in Ajinkya et al. (2005, 370) suggesting structural differences in the determinants of the error and bias in managers' forecasts of quarterly *vis-à-vis* annual earnings, our analysis confirms that managers' incentives for issuing (shorter-horizon) forecasts of quarterly EPS differ fundamentally from their incentives in issuing (longer-horizon) forecasts of annual earnings. First, managers' forecasts of annual earnings are optimistic relative to *ex post* actual EPS (Table 3), but we find that their forecasts of quarterly earnings are on average pessimistic ($p \leq 0.01$), consistent with Bergman and Roychowdhury's (2008) conclusion that long-horizon disclosure choices reflect managers' desire to maintain optimistic earnings valuations, whereas they issue pessimistic short-term forecasts to set beatable expectations. Second, Rogers and Stocken (2005) model managers' incentives to issue biased (long-horizon) *initial forecasts of annual EPS*. They find support for four of their five predictions for managerial incentives (and we find support for these same four predictions in our sample of annual forecasts). When we apply their model to managers' (shorter-term) forecasts of quarterly EPS, three of the five predictions are *not* supported, and the remaining two are supported only at the 10 percent level, thus confirming that managers' incentives to bias forecasts of annual earnings differ from their incentives to bias forecasts of quarterly earnings.

It is therefore not surprising that the frequency of heaping in quarterly forecasts is only about 60 percent of that in annual forecasts. Nor is it surprising that our model of the determinants of heaping yields mixed results when applied to quarterly forecasts. Proprietary information costs remain significant in the expected direction at $p \leq 0.05$. Predicted bias is significant in the expected direction at $p \leq 0.05$ using the more powerful ordered probit, in the subsample of first forecasts of quarterly earnings (those with the longest horizons). Uncertainty is not significant.

Evidence that uncertainty, proprietary information costs, and predicted bias play the most significant role in spurring heaping where theory suggests they would have the most powerful effect (in the longest-horizon initial forecasts of annual EPS), a moderate but still significant role in shorter-horizon revisions of annual EPS forecasts, and the least role where theory suggests they would have least effect (in the shortest-horizon forecasts of quarterly EPS) further supports the validity of our inferences. Evidence that forecast horizon moderates not only managers' tendency to heap, but that it also moderates the effects of uncertainty, proprietary information costs, and predicted bias on managers' tendency to heap, responds to the Hirst et al. (2008) call to explore how moderator variables (in our case, forecast horizon) affect the magnitude of main effects, to establish boundary conditions where main effects are (and are not) reliably observed.

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[23] Focusing on forecasts of annual earnings is also relevant given firms are switching from providing short-term guidance on quarterly earnings to longer-term guidance on annual earnings (e.g., Plitch 2006).

Finally, our consequences tests focus on how heaping in managers' forecasts affects revisions in *analysts' forecasts*. To examine whether security prices act as if *investors* understand managers' nickel forecasts are less accurate, we estimate an analog to Model 4, substituting two-day abnormal returns at the management forecast date as the dependent variable. Inferences are similar to those based on revisions in analysts' forecasts: investors place less weight on good news nickel forecasts than non-nickel forecasts, even after adjusting for predicted bias.

VII. CONCLUSIONS

We identify a new systematic wedge between managers' predictions about their firms' earnings performance and firms' *ex post* realized results: Nearly *half* of managers' forecasts of annual EPS end in nickel intervals, even though only around 20 percent of subsequent *actual* reported EPS end in nickel intervals. Prior research considers this type of heaping at common intervals to be a heuristic response to uncertainty. However, we find that managers' nickel forecasts are not only less accurate, but they are also more optimistically biased than their non-nickel forecasts. Consequently, uncertainty cannot be the sole explanation for heaping in management forecasts.

We therefore test economic explanations for managers' propensity to issue forecasts that heap at nickel intervals. We find managers' forecasts are more likely to heap when: (1) there is more uncertainty about earnings, (2) the firm has higher proprietary information costs, and (3) managers have stronger incentives to upward-bias their forecasts and it is difficult for the market to assess the truthfulness of the forecast. These results suggest heaping in management forecasts stems not only from a benign psychological heuristic response to uncertainty, but also from managers' strategic incentives—especially efforts to protect the firm's proprietary information, and managers' self-serving opportunism in response to their own economic incentives. To the best of our knowledge, this is the first evidence that strategic economic incentives (as distinct from benign psychological heuristics) help explain heaping. Our inferences hold after Reg FD and Sarbanes-Oxley, so heightened scrutiny of corporate disclosures has not deterred managers from issuing optimistically biased and less accurate nickel forecasts when they have economic incentives to do so.

These economic forces play more significant roles in managers' longer-horizon forecast choices (i.e., initial forecasts of annual earnings) than in their shorter horizon forecasts. This is because longer horizon forecasts are subject to more uncertainty, longer horizons extend the period over which managers can benefit from opportunistically biased forecasts, and longer horizons extend the period over which the firm potentially incurs proprietary information costs if it accelerates disclosure of information that competitors can use against it.

We then explore the consequences of heaping. When managers issue forecasts heaped at nickel intervals, this spurs heaping even among active analysts. Thus, we identify an additional determinant of heaping in *analysts'* forecasts that complements Herrmann and Thomas' (2005) conclusion that heaping in *analysts'* forecasts is primarily attributable to analysts with fewer resources or lower ability. We also find that when management forecasts convey good news, analysts place less weight on (noisier) nickel forecasts than on non-nickel forecasts (even after controlling for the additional predicted bias), but analysts do not appear to discount bad news nickel management forecasts. Finding greater adjustment for noise and bias in good news nickel management forecasts highlights a new setting in which market participants appear to take bad news forecasts at face value, while evaluating good news forecasts more skeptically.

In a recent review, Hirst et al. (2008) conclude that surprisingly little is known about the detailed characteristics (such as precision or specificity) of management forecasts. Our study helps address this void in the literature by providing evidence that "not all point forecasts are created equal." Finding that managers' propensity to issue nickel forecasts increases with both the firm's proprietary information costs and managers' incentives to issue biased disclosures suggests that managers' choice between nickel and non-nickel disclosures is a finer element of the forecast specificity decision that has not been previously recognized in the literature. We contribute by not only identifying this finer choice element, but also by exploring determinants and consequences of this choice. Our results suggest that future research on the specificity of management forecasts may benefit from refining the traditional measures of forecast precision by delineating nickel versus non-nickel point forecasts, rather than treating all point forecasts as if they were equally precise. Finally, our evidence that managers' nickel forecasts of annual earnings are more optimistically biased and less accurate than their non-nickel forecasts, and that analysts do not appear to fully adjust for this additional optimism and noise, especially in managers' bad news forecasts, is also of interest from a practical perspective.

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Comprehensive Income: Who's Afraid of Performance Reporting?

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ABSTRACT: Firms can report comprehensive income in either an income-statement-like performance statement or the statement of equity. Traditional theories of contracting incentives cannot explain this reporting *location* choice that only affects *where* comprehensive income data appear, because the contractible values of net income, other comprehensive income items, and comprehensive income are exactly the same regardless of the location where the firm reports comprehensive income. Drawing on theory, analysis of comment letters, and results of survey-based and behavioral research, we identify two factors—equity-based incentives and concerns over job security—that help explain why most firms do *not* follow policymakers' preference to report comprehensive income in a performance statement. Our empirical evidence on a broad cross-section of firms shows that managers with stronger equity-based incentives and less job security are significantly less likely to use performance reporting. Overall, our study suggests that even though the reporting location choice is inconsequential in a traditional rational markets view, managers act as if they believe that comprehensive income reporting location matters.

Keywords: *accounting choice; comprehensive income; executive compensation; managerial job security.*

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I. INTRODUCTION

Financial reporting standards in the U.S. currently allow firms to report comprehensive income in either an income-statement-like format as part of a statement of performance,[1] or in the statement of equity (Financial Accounting Standards Board [FASB] FAS No. 130). Firms report exactly the same information (i.e., the same values of net income, other comprehensive income, and comprehensive income) either way. It is only the reporting location that differs. In traditional models of financial markets, rational investors fully process information regardless of its location, so it does not matter where firms report comprehensive income.

Standard-setters, however, believe the location of comprehensive income does matter. FAS No. 130 stated a preference for an income-statement-like presentation—referred to as performance reporting—viewing this as the more transparent presentation (FASB 1997, paragraph 67).[2] Managers say developing a reputation for transparent reporting is a key factor in their disclosure decisions (Graham et al. 2005, 54), so we would expect managers who believe reporting location has little consequence to follow policymakers' recommendations and use performance reporting. Yet most firms report comprehensive income in the statement of equity, contrary to policymakers' preferences (e.g., Bhamornsiri and Wiggins 2001; Pandit et al. 2006).

We investigate determinants of managers' comprehensive income reporting location choices. The only empirical evidence on this choice is the Lee et al. (2006) investigation in the property-liability insurance industry. They provide evidence that insurers who report comprehensive income in a statement of equity are more likely to smooth earnings by cherry-picking realized gains and losses on available-for-sale (AFS) securities. Apparently, these insurers believe performance reporting would make their attempts to smooth earnings more transparent. The insurance industry is an excellent setting in which to investigate the link between firms' comprehensive income reporting choices and selective sales of AFS securities. Opportunities to cherry-pick are ripe in this industry because the average AFS portfolio is about 40 percent of total assets (Godwin et al. 1998).

Outside the insurance industry, AFS securities are a smaller proportion of firm assets, which reduces opportunities for cherry-picking. Yet firms outside the insurance industry are even less likely to use performance reporting (Lee et al. 2006), so cherry-picking cannot be the primary reason non-insurers report comprehensive income in the statement of equity. Nor can traditional contracting incentives explain a choice that is merely one of *location*—the values of net income, other comprehensive income, and comprehensive income are exactly the same regardless of where comprehensive income appears. So contracts relying on recognized contractible accounting numbers (e.g., debt contracts whose covenants draw on net income or its components) are not affected. Consequently, we must look beyond traditional contracting incentives to identify other reasons that can explain the comprehensive income reporting location decision for a broad cross-section of firms.

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[1] Under FAS No. 130, *Reporting Comprehensive Income,* the income-statement-like format can take one of two specific forms: (1) a statement that includes the information in an income statement as well as comprehensive income, or (2) a separate statement that begins with net income and ends with comprehensive income. Following Lee et al. (2006), in our primary analyses we group all firms using the income-statement-like format in one category. Later sensitivity tests consider the two forms separately.

[2] FAS No. 130 does not explicitly define the term *transparent,* but we consider a transparent presentation to be one that is easily recognized or detected, and we consider transparent reporting to be open and frank, consistent with definitions provided by *Dictionary.com Unabridged (v 1.1).* Random House, Inc. http://dictionary.reference.com/browse/transparent (accessed: March 23, 2009).

As described in the next section, analysis of the comment letters responding to the FASB's controversial Exposure Draft, *Reporting Comprehensive Income* (FASB 1996), which called for mandatory performance reporting, suggests many managers believed reporting comprehensive income in a salient performance statement would lead investors and other stakeholders to increase their assessments of the volatility of the firm's performance. Behavioral finance theory (Hirshleifer and Teoh 2003) and the results of laboratory experiments (Maines and McDaniel 2000) support managers' stated concerns that more salient performance reporting would likely adversely affect investors' perceptions of the volatility of the firm's performance.

Managers responding to the Graham et al. (2005) survey also say they believe higher perceived volatility of firm performance could hurt the firm's stock price and assessments of the manager's own performance. We therefore hypothesize that managers with more powerful equity-based incentives (whose wealth would suffer more from lower stock prices) and managers with less job security (whose jobs are at greater risk if the firm experiences unfavorable future performance) would be less likely to report comprehensive income in a performance statement.

We test our expectations on the initial comprehensive income reporting choice made by Standard & Poor's (S&P) 500 firms during the 1998 to 2001 period. Consistent with our hypotheses, firms that do not follow policymakers' preferences for performance reporting are headed by CEOs with more powerful equity-based incentives and less job security.[3] To assess the robustness of our results beyond (large) S&P 500 firms, we also examine the initial comprehensive income reporting location choices of a random sample of 181 firms not included in the S&P 500. Our inferences generalize to these smaller firms. An additional analysis of the small set of S&P 500 firms that changed their comprehensive income reporting location further supports our inferences: firms whose CEOs have increasingly powerful equity incentives and decreasing job security are more likely to switch away from performance reporting. This "change" analysis suggests our primary conclusions are not attributable to some unidentified correlated omitted variable or uncontrolled firm-specific effect.

We also find that firms with larger (absolute) unrealized gains and losses on AFS securities are less likely to use performance reporting. Thus, firms with more opportunity to manage earnings by selectively selling AFS securities tend to avoid performance reporting, consistent with evidence in Hunton et al. (2006) that managers believe performance reporting increases the likelihood users detect such opportunistic sales.

Our study contributes to the literature in several ways. First, weaving together theory, prior research on comment letters, prior survey-based research, and results of behavioral research, we identify new determinants of managers' choice of where to *locate* recognized accounting numbers. This focus on a pure *location* choice fundamentally differs from most other accounting studies that examine choices affecting reported values of recognized accounting numbers.[4] Our evidence that managers with more powerful equity incentives and less job security are less likely to use performance reporting appears to reflect managers' concerns that more salient reporting of comprehensive income could hurt stock prices and evaluation of their performance.

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[3] Following prior research on determinants of accounting choice (e.g., Cheng and Warfield 2005; Bergstresser and Philippon 2006), we focus on the CEO's incentives, because the CEO's preferences likely affect the firm's accounting choices, even if the CFO or controller makes the choice. For example, if the CEO prefers smoother reported performance, the firm's accounting choices are likely to further this goal.

The evidence suggests that even though the reporting location choice is inconsequential in a traditional efficient markets view, managers act as if they believe it matters. Evidence that managers' personal equity-based incentives and job security concerns are associated with comprehensive income reporting location choices in a sample that is broadly representative of firms in the U.S. economy complements the Lee et al. (2006) evidence that cherry-picking sales of AFS securities is associated with this choice in the insurance industry, where opportunities to cherry-pick are more pervasive. Collectively, this evidence on who ignores the FASB's recommendations is relevant as the FASB and IASB are considering requiring all firms to prepare a single performance statement titled "Statement of Comprehensive Income," which would essentially require performance reporting (FASB 2008; IASB 2008).

Second, our study contributes by providing evidence suggesting that managers' job security concerns affect their financial reporting choices. While prior research has made this conjecture (e.g., Fudenberg and Tirole 1995), as far as we are aware, the only previous related evidence is the Graham et al. (2005) *survey* results suggesting managers' career concerns affect their reporting choices. This dearth of empirical evidence prompts Graham et al. (2005, 24) to characterize the effects of managers' career concerns on their financial reporting choices as an "under-explored" issue. Our study begins to address this void.

Third, we extend prior research showing that equity-based incentives affect earnings management (e.g., Cheng and Warfield 2005; Bergstresser and Philippon 2006) by showing that equity incentives also affect a transparency-related accounting choice—the decision to disclose accounting information in a more or less salient location.

II. DEVELOPMENT OF HYPOTHESES

An extensive literature focuses on determinants of accounting choices that affect the value of recognized accounting numbers. Choices about accruals (e.g., Healy 1985; Holthausen et al. 1995), employee stock options (e.g., Aboody et al. 2004), and off-balance-sheet financing (e.g., El-Gazzar et al. 1986) all affect reported income. Traditional contracting incentives are often used to explain these kinds of choices that affect the values of accounting numbers used in contacts. For example, debt covenants often incorporate recognized accounting amounts such as net income and debt-to-equity ratios, so incentives stemming from debt contracts help explain accounting choices that affect recognized accounting numbers (e.g., Press and Weintrop 1990; DeFond and Jiambalvo 1994).

But the choice we study is fundamentally different: the only difference is the *location* where these numbers are reported. Therefore, we look beyond the contracting literature and draw on behavioral finance, comment letters, and survey-based and behavioral research to identify a parsimonious set of factors that we expect to explain most firms' decisions *not* to follow policymakers' preference for performance reporting.

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[4] Our investigation of *determinants* of a *location* choice differs fundamentally from prior research on choices between recognition versus disclosure, which: (1) do affect the value of reported net income and other contractible accounting numbers, and (2) mostly focuses on *consequences* of this choice (e.g., Amir 1993; Aboody 1996; Barth et al. 2003; Ahmed et al. 2006). Cotter and Zimmer (2003) is a rare exception that investigates *determinants* of a recognition versus disclosure choice.

Figure 1 summarizes our hypothesis development. We start by reviewing evidence that managers likely believed performance reporting would lead financial statement users to perceive the firm's performance as more volatile. Then we explain why managers would expect an increase in the perceived volatility of firm performance to hurt the firm's stock price and their own performance evaluation. We use these discussions to develop our hypotheses that managers who avoid performance reporting have stronger equity-based incentives and less job security.

Comprehensive income includes unrealized gains and losses on (1) AFS securities; (2) foreign currency translations; (3) minimum pension obligations; and (4) certain hedging and derivative activities. These unrealized gains and losses stem from uncontrollable and volatile market forces (e.g., stock market trends and changes in currency exchange rates and interest rates) and are therefore transitory.[5] It is not surprising that comprehensive income is generally more volatile than net income (Barth et al. 1995; and our Figure 2). Comprehensive income is an incomplete measure of the firm's performance because unrealized gains and losses on other assets and liabilities of the firm, which may be natural or planned hedges for the unrealized gains and losses included in other comprehensive income, are not recognized. Thus, other comprehensive income items are generally volatile, transitory, and incomplete representations of the firm's unrealized gains and losses.

In a theoretical model of financial statement users with limited attention and information-processing ability, Hirshleifer and Teoh (2003; hereafter HT) suggest that more salient reporting of volatile, transitory, and incomplete information will increase users' assessment of the volatility of the firm's performance. HT argue that users more fully assimilate information that is more salient and fail to fully assimilate relevant information that is less prominent. Their argument suggests that if comprehensive income is more salient, then: (1) users will put more weight on comprehensive income as a performance measure than they would if it were less salient (Bloomfield's [2002] *incomplete revelation hypothesis* makes a similar prediction), and (2) users will fail to consider offsetting unrealized gains and losses on other assets and liabilities that are not recognized in the current accounting model.[6] Both effects will lead users to believe the firm's performance path is more volatile.[7]

Evidence from comment letters and laboratory experiments support the implication of HT's theory that salient performance reporting leads investors to assess the firm's performance as more volatile. The 1996 Exposure Draft proposing mandatory performance reporting spurred over twice the average number of comment letters as the FASB's first 100 accounting standards (Yen et al. [2007, 59] citing Tandy and Wilburn [1992]). Yen et al. (2007) find that over 80 percent of the comment letters expressed concern about financial statement users' reactions to mandatory performance reporting. Nearly half expressed concerns over the volatility of comprehensive income. Many argued that performance reporting

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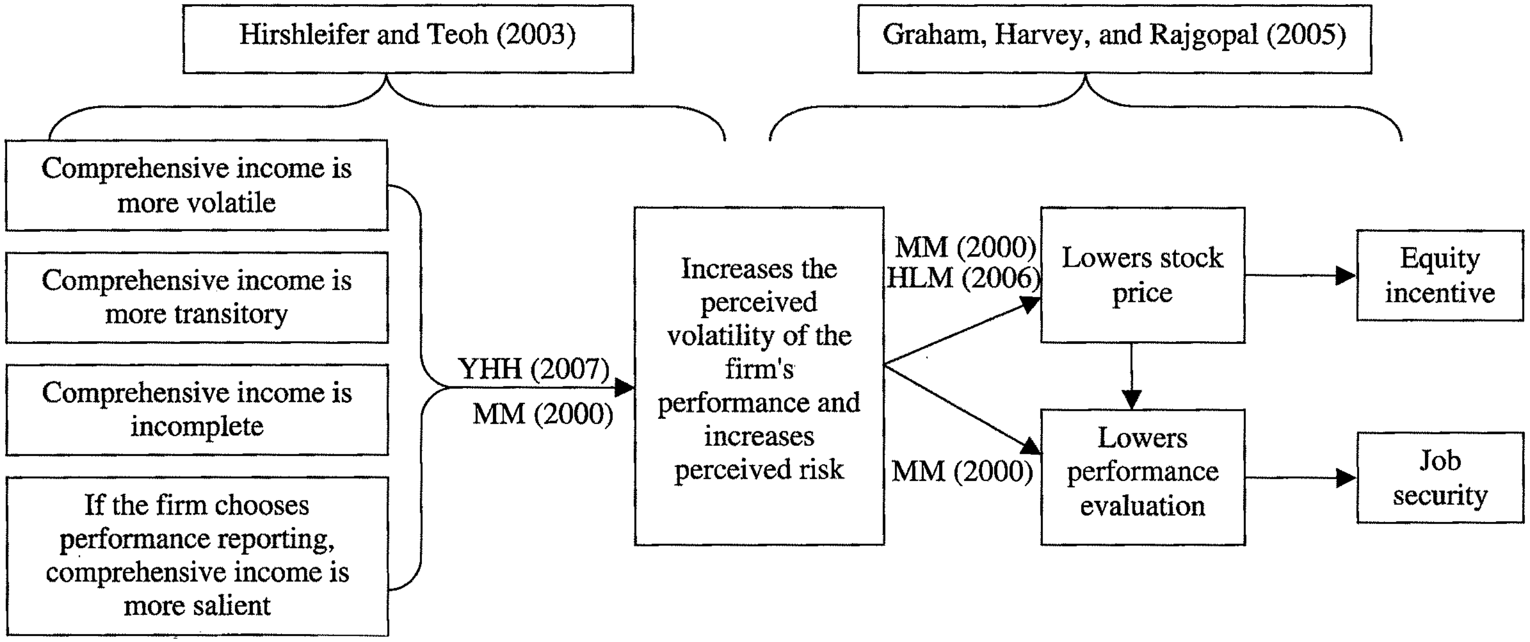
[5] In our sample, the average of the firm-specific serial correlations (over 1998 to 2006, which spans both bear and bull markets) in other comprehensive income is -0.10, whereas the average serial correlation in net income is 0.26.

[6] Bloomfield et al. (2006) provide evidence consistent with HT's second proposed effect. Using experimental financial markets, they show that when a firm's other comprehensive income is highly correlated with other public information about the firm (for example, when the unrealized gains and losses on the firm's AFS securities are correlated with the firm's returns), investors do not fully recognize the redundancy of the information in other comprehensive income and the firm's stock price is influenced by *where* the firm reports comprehensive income.

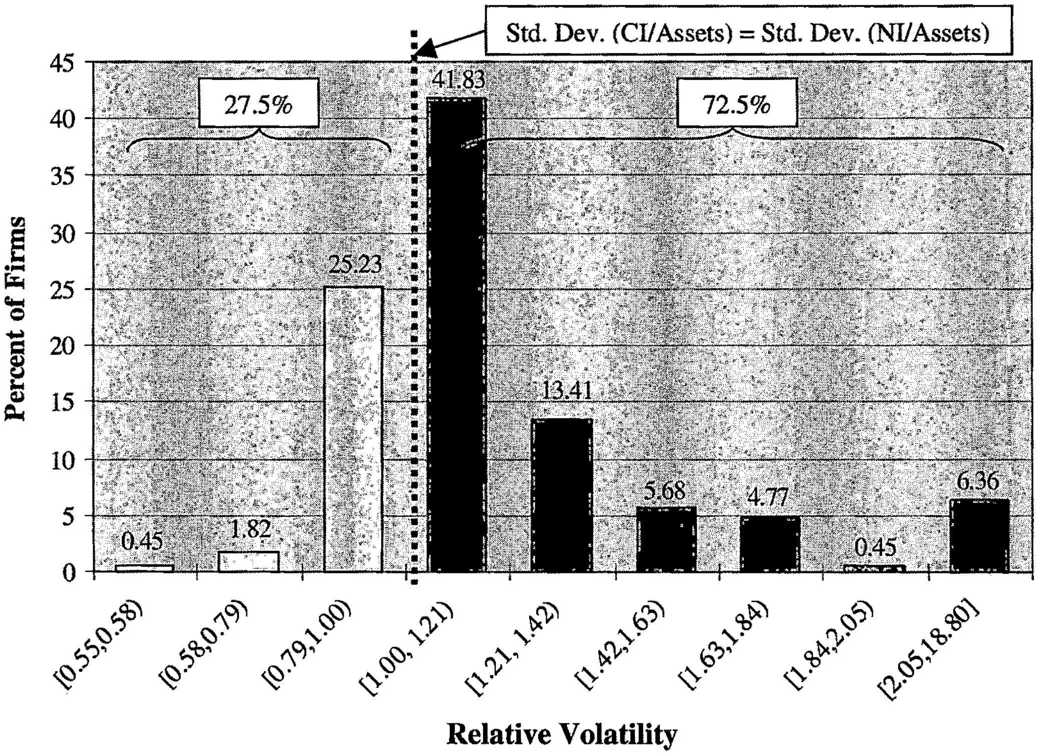
[7] HT (2003, 380) note that their model "suggests that firms that hedge may be viewed by investors as more risky than those that do not if hedge profits are marked-to-market whereas the long-term business risk the firm is hedging is not marked to market."

FIGURE 1
Development of Hypotheses


Hirshleifer and Teoh (2003)
Graham, Harvey, and Rajgopal (2005)
Comprehensive income is more volatile
Comprehensive income is more transitory
Comprehensive income is incomplete
If the firm chooses performance reporting, comprehensive income is more salient
YHH (2007)
MM (2000)
Increases the perceived volatility of the firm's performance and increases perceived risk
MM (2000)
HLM (2006)
MM (2000)
Lowers stock price
Lowers performance evaluation
Equity incentive
Job security

YHH = Yen, Hirst, and Hopkins (2007);
MM = Maines and McDaniel (2000); and
HLM = Hunton, Libby, and Mazza (2006).

FIGURE 2
Volatility in Comprehensive Income Relative to Net Income



$$\text{Relative Volatility} = \frac{\text{Standard Deviation of (Comprehensive Income/Assets)}}{\text{Standard Deviation of (Net Income/Assets)}}$$

would cause investors to perceive the firm as riskier and asserted that companies would change their operations to reduce the volatility of comprehensive income "to 'head off' a negative user reaction" (Yen et al. 2007, 69).[8]

The FASB backed down on mandatory performance reporting without providing an explicit rationale (FASB 1997, dissent). Paragraph 60 of FAS No. 130 says respondents to the Exposure Draft expressed concern: (1) about highlighting uncontrollable volatility in other comprehensive income items, (2) that comprehensive income is more a measure of entity performance than a measure of management performance, and (3) that users would be confused by two measures of income. The dissent to FAS No. 130 points out that the confusion argument appears to be an excuse, so it appears the FASB reversed itself as a result of pushback from constituents, largely based on concerns that more salient presentation of other comprehensive income items would highlight volatility due to market forces beyond management's control.

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[8] We independently analyzed the 97 comment letters that managers of our sample firms sent in response to the initial exposure draft. Each letter opposed performance reporting, often expressing concern that performance reporting would increase perceived volatility of the firm's performance. Managers' actions were consistent with their comment letters, as each of these 97 firms actually chose not to use performance reporting.

Laboratory experiments also suggest financial statement users judge firms' performance as more volatile only if comprehensive income appears in a performance statement. Maines and McDaniel (2000, 179) find "investors' judgments of corporate and management performance reflect the volatility of comprehensive income only when it is presented in a statement of comprehensive income." Similarly, Hirst and Hopkins (1998) find that when comprehensive income appears in the statement of equity, half of the experienced and sophisticated financial analyst participants do not recall seeing the term *comprehensive income.*[9] Finally, Hunton et al. (2006) report that managers participating in their experiment act as if they believe the location of comprehensive income affects the extent to which financial statement users attend to the information. Their evidence supports our maintained assumption that managers believed the location of comprehensive income would likely affect investors' perceptions.

In sum, we expect that when managers were making their initial location decision, they *expected* performance reporting would lead users to assess the volatility of the firm's performance as higher. For this maintained assumption to hold, all that is relevant is *what managers believed at the time they made their initial choice.* Although our sample managers' expectations are not directly observable today, all the evidence available at the time supports our assumption.[10] Note our maintained assumption does not require that managers' *ex ante* beliefs in 1998 turn out to be accurate descriptions of investors' *ex post* reactions.[11] If our assumption is incorrect, then this would work against finding the hypothesized effects.

Evidence that Managers Would Expect an Increase in the Perceived Volatility of Firm Performance to Hurt Stock Prices and Their Performance Evaluations

The Graham et al. (2005; hereafter GHR) survey evidence shows managers believe that—even keeping cash flows constant—stakeholders perceive more volatile performance measures as indicating higher firm risk. Over 88 percent of their surveyed CFOs responded that investors perceive smoother earning paths as less risky. This concern is echoed in the comment letters on the Exposure Draft (Yen et al. 2007), and evidence from laboratory experiments confirms that both professional and nonprofessional investors associate variability in earnings with higher firm risk (Farrelly et al. 1985; Lipe 1998). Koonce et al. (2005) also show that financial statement users perceive uncontrollable items as increasing risk, so the relatively uncontrollable nature of other comprehensive income items likely exacerbates users' perceptions of firm risk.

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[9] Hirst et al. (2004) find that even financial analysts with industry experience distinguish banks' exposure to interest-rate risk only when unrealized gains and losses from all the bank's financial assets and liabilities appear in a performance statement. Results in Hirst and Hopkins (1998) and Hirst et al. (2004) are consistent with Brown's (1997) survey evidence that analysts regard the statement of equity as one of the least useful components of the annual report.

[10] We focus on evidence about managers' *expectations* at the time they made their initial choice. Absent perfect foresight, one would not expect managers to perfectly predict how investors will react to new disclosures. Moreover, Amershi and Sunder's (1987) rational expectations model suggests that *even in equilibrium in an efficient market,* managers may continue to hold incorrect beliefs about investors' use of accounting information.

[11] Whether managers' expectations turn out to be correct *ex post* is beyond the scope of our study. In an analysis that is secondary to their main contribution, Chambers et al. (2007) conclude that reporting location is not associated with a significant difference in the pricing of other comprehensive income items over the period 1998–2003. However, their analysis does not control for self-selection of the reporting location, which could bias the pricing multiples, nor does it explicitly consider the pricing of the component of other comprehensive income that represents the reclassification of realized gains and losses. We therefore believe that further research on the pricing implications of the location choice is warranted.

GHR conclude that managers believe higher perceived risk hurts the firm's stock price, with the interview evidence leading to the persistent theme, "the market hates uncertainty" (GHR 2005, 47). Many surveyed managers feared their P/E multiple would drop if earnings become more volatile. This concern is supported by research linking smoother earnings paths with higher P/E multiples (e.g., Barth et al. 1999; Francis et al. 2004). When pressed why they believed earnings volatility matters, GHR's managers say it creates uncertainty. The idea is that uncertainty creates a perceived estimation risk in expected returns (e.g., Klein and Bawa 1976; Jorion 1985; Xia 2001) that causes investors to demand a higher risk premium, which in turn hurts stock price. Similarly, accounting research suggests that poor earnings quality (greater variation in/lower persistence of earnings or its components) is priced as if it contributes to information risk (e.g., Francis et al. 2004; Aboody et al. 2005). GHR also point out that because managers believe volatile earnings lead to volatile returns, they also believe idiosyncratic volatility is priced, consistent with evidence in Goyal and Santa-Clara (2003). Behavioral research also supports managers' concerns that more salient performance reporting could hurt the firm's stock price (e.g., Maines and McDaniel 2000; Hunton et al. 2006).

In addition to expecting the above adverse effects on equity investors, GHR's managers also believe that more volatile earnings paths lead to: (1) lower credit ratings and higher costs of debt; (2) greater customer and supplier concern about the firm's viability, and thus inferior terms of trade; and (3) lower growth projections by investors, all of which negatively affect the firms' stock price. Empirical research provides some support for these additional concerns, by linking smoother earnings paths with: (1) better credit ratings and lower cost of debt (e.g., Gu and Zhao 2006), and (2) better terms of trade with customers (e.g., Sommer 1996).

Managers likely expect lower stock prices to hurt their performance evaluations. Furthermore, higher perceived riskiness of the firm can directly hurt managers' own performance evaluations: Maines and McDaniel (2000) find that investors evaluating managers' performance penalize managers for volatility in comprehensive income only when comprehensive income appears in the salient performance statement (and not when it appears in the statement of equity).

Hypotheses

Figure 1 summarizes the support for our expectations that managers *believed* reporting comprehensive income in a more salient performance statement could lower both stock price and managers' performance evaluations. We expect managers with the most to lose from lower stock prices and poorer performance evaluations will be less likely to use performance reporting.

Managers whose wealth is more sensitive to changes in the firm's stock price (i.e., managers with more powerful equity-based incentives) have more to lose from a lower stock price, and thus likely prefer reporting methods that reduce the perceived volatility of firm performance (Goel and Thakor 2003). Our first hypothesis (stated in the alternative) is:

> **H1:** The likelihood that a firm avoids reporting comprehensive income in a performance statement increases in the power of the CEO's equity-based incentives.

Managers who face greater risk of losing their jobs if they receive unfavorable performance evaluations—including repercussions of poor stock price performance—have less job security. We hypothesize that managers with less job security have more to lose from increased perceived volatility of firm performance. CEOs are rightfully concerned about job security because dismissals increased significantly from 1971 to 1994 (Huson et al. 2001) and further increased by 170 percent from 1995 to 2003 (Lucier et al. 2004). Our second hypothesis (stated in the alternative) is:

H2: The likelihood that a firm avoids reporting comprehensive income in a performance statement decreases as the CEO's job security increases.

Both hypotheses propose determinants of location choice that reflect costs of performance reporting. The benefit of following policymakers' preference for performance reporting is that it helps managers earn a reputation for transparency. To control for the extent to which the firm acts as if it values transparency (similar to Lee et al. [2006]), our empirical tests include a control for the firm's propensity for transparent reporting practices.

III. RESEARCH METHOD

Sample Selection and Descriptive Statistics

We start with the S&P 500 firms as of December 1998 from Compustat's Price, Dividend, and Earnings file. 1998 is the required adoption year for FAS No. 130. We drop 46 firms for which we cannot reliably identify comprehensive income or obtain executive compensation data on ExecuComp from 1998 to 2001. To more cleanly isolate the determinants of firms' long-term comprehensive income reporting choices, we follow Lee et al. (2006) and drop 14 firms that changed their location choice between 1998 and 2001.[12] Thus, we investigate the choices of the remaining 440 firms. This broad cross-sectional sample is representative of large firms in the U.S. economy and are of interest in their own right, as they comprise 66 percent of the total market value of NYSE, AMEX, and NASDAQ.

Only 19 percent of our sample (85 firms) reports comprehensive income in a performance statement (with the remaining 81 percent [355 firms] reporting in a statement of equity), whereas Lee et al. (2006) find that approximately half of their sample of property-liability insurers use performance reporting. Therefore, as Lee et al. (2006, 687) note, one should use caution in generalizing from the property-liability industry to the broader population.

Table 1 displays descriptive statistics for the first year the firms reported comprehensive income. Other comprehensive income is material for the mean (median) firm: the absolute value of other comprehensive income is 12.7 percent (7.2 percent) of the absolute value of net income. To provide evidence on the relative magnitudes of each of the four components of other comprehensive income, we divide the absolute value of each component by the sum of the absolute values of all four components. We also record the number of firms reporting a nonzero value for each component. The most common component is unrealized gains and losses on foreign currency translation (*FORCUR*), reported by 81 percent of our sample firms. This component represents more than half of other comprehensive income for the mean and median firm. The next most common component is unrealized gains and

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[12] The low rate of switches confirms that the comprehensive income reporting choice is a long-term disclosure commitment. We later examine the small set of firms that changed their comprehensive income reporting location.

TABLE 1
Descriptive Statistics for Other Comprehensive Income in the Year 440 S&P 500 Firms Initially Report Comprehensive Income

	Mean	Std. Dev.	Lower Quartile	Median	Upper Quartile
$\frac{\text{Absolute value of other comprehensive income}}{\text{Absolute value of net income}}$	0.127	0.221	0.027	0.072	0.142
$\frac{\text{Absolute value of } \mathit{MKTSEC}}{\text{Sum of the absolute values of other comprehensive income items}}$ (n > 0)/440 = 60%	0.309	0.385	0.000	0.083	0.691
$\frac{\text{Absolute value of } \mathit{PENSION}}{\text{Sum of the absolute values of other comprehensive income items}}$ (n > 0)/440 = 39%	0.113	0.242	0.000	0.000	0.088
$\frac{\text{Absolute value of } \mathit{FORCUR}}{\text{Sum of the absolute values of other comprehensive income items}}$ (n > 0)/440 = 81%	0.554	0.407	0.075	0.700	0.954
$\frac{\text{Absolute value of } \mathit{DERIVATIVES}}{\text{Sum of the absolute values of other comprehensive income items}}$ (n > 0)/440 = 4%	0.022	0.135	0.000	0.000	0.000

Descriptive statistics are from the initial comprehensive income reporting year, over the period 1998 to 2001. *MKTSEC* is the unrealized gains and losses from the firm's available-for-sale securities. *PENSION* is the unrealized gains and losses resulting from changes in the minimum pension obligation. *FORCUR* is the unrealized gains and losses from foreign currency translation. *DERIVATIVES* is the unrealized gains and losses related to hedging and derivative activity.

losses on AFS securities (*MKTSEC*), reported by 60 percent of our sample firms.[13] For the mean (median) firm this component represents more than 30 percent (8 percent) of other comprehensive income. Less than 40 percent of our sample report a pension component (*PENSION*),[14] and less than 4 percent report a derivatives component (*DERIVATIVES*) because most firms started reporting comprehensive income before FAS No. 133 (FASB 1998) became effective in 2001. Because the derivative component is so rare, we do not consider it separately.

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[13] Additional analysis confirms that opportunities for cherry picking are less abundant outside the insurance industry, so cherry picking is less likely to be the primary determinant of reporting location choice for our sample than for the insurers examined by Lee et al. (2006). All our insurers report unrealized gains and losses on AFS securities but only about half of the noninsurance firms do so, and the average magnitude of the unrealized gains and losses on AFS securities is over three and a half times larger among the insurers than the rest of the sample.

[14] These data precede FAS No. 158 (FASB 2006), which increases the pension component by requiring recognition in comprehensive income of changes in the over- or under-funded status of defined-benefit postretirement plans.

Our maintained assumption is that at the time managers make the initial comprehensive income reporting decision they *expect* the *future stream* of comprehensive income will be more variable than the future stream of net income. To assess the accuracy of this expectation, we compare the *ex post* volatility of comprehensive income to net income for each sample firm. Over the period 1996 to 2004, we compute the standard deviation of each series of income scaled by assets.[15] We then divide the standard deviation of scaled comprehensive income by the standard deviation of scaled net income. As expected, comprehensive income is 32 percent (7 percent) more volatile than net income for the mean (median) firm. Figure 2 displays the distribution of the volatility of comprehensive income relative to net income. The shaded bars to the right of the dotted line show that for 72.5 percent of the sample firms, comprehensive income actually turns out to be more volatile than net income, *ex post*. This supports our maintained assumption that the average manager would expect comprehensive income to be more volatile than net income.

Empirical Proxies for CEO Reporting Incentives

Power of the CEO's Equity-Based Incentives

We hypothesize that managers with more powerful equity-based incentives are more likely to avoid performance reporting. We use Bergstresser and Philippon's (2006) measure of the sensitivity of the CEO's stock and stock option holdings to changes in stock prices. Bergstresser and Philippon (2006) measure the dollar change in the value of a CEO's stock and stock option holdings arising from a one percentage point increase in the firm's stock price:

ONEPCT = the effect of a one percentage point increase in the firm's stock price on the value of the firm's shares held by the CEO (i.e., 0.01 × share price × number of shares the CEO owns) *plus* the effect of a one percentage point increase in the firm's stock price on the value of the CEO's options, calculated following Core and Guay (2002).[16]

Bergstresser and Philippon (2006) then deflate *ONEPCT* to construct *EQUITY_INC*, and argue that *EQUITY_INC* reflects the portion of the CEO's total annual compensation from the firm stemming from a one percentage point increase in the firm's stock price:

$$EQUITY\_INC = \frac{ONEPCT}{ONEPCT + SALARY + BONUS} \quad (1)$$

where *SALARY* is the CEO's salary, and *BONUS* is the CEO's cash bonus. We obtain stock and stock option ownership information, and salary and bonus amounts from ExecuComp, measured at the end of the first year the firm reports other comprehensive income.

Our expectation that managers with more powerful stock and stock option-based equity incentives prefer less volatile *reported earnings* is not inconsistent with traditional finance theory where owners grant stock option compensation to give risk-averse managers incentives to assume more risk in *operational and investment decisions.*[17] Options may stimulate managers to make riskier operating and investment decisions, but we are unaware of theory suggesting options give managers incentives to make *financial reporting choices* that inflate the volatility of *reported earnings.* That is, managers do not lobby the FASB to adopt standards that add uncontrollable volatility to reported income. To the contrary, most managers prefer less volatile earnings streams (GHR 2005). Similarly, Cheng and Warfield (2005) argue that CEOs with large equity incentives prefer smoother earnings series, likely because managers believe more volatile earnings streams reduce the value of the firm's shares (GHR 2005). In any event, our inferences remain the same when we omit option holdings from our measure of equity incentives.

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[15] Throughout the study we use *as-reported* comprehensive income data. We are able to obtain these data for two years before the year the firm initially reports comprehensive income, because FAS No. 130 requires that in the initial reporting year firms must also disclose comparative comprehensive income data for the previous two years.

[16] Core and Guay (2002) show their procedure leads to unbiased estimates of the sensitivity of option value to changes in stock prices of firms whose options are in-the-money, and these estimates capture more than 99 percent of the variation in option portfolio value. Only three of our sample firms have out-of-the-money unexercisable options, and omitting them does not affect our inferences.

Job Security

Our second hypothesis is that CEOs whose jobs are less secure are more likely to avoid performance reporting. Consistent with GHR's (2005, 24) characterization of the effect of job security on managers' financial reporting choices as "under-explored," the literature in finance, management, and accounting does not yield a consensus empirical measure of job security. While the human resources literature has investigated job security, those studies use self-reported survey responses. It is not possible to survey managers about their perceptions of their job security when they made the initial comprehensive income reporting decision, so we must develop our own proxy.

The human resources literature defines job security as the perceived stability and continuance of one's job, especially in a threatened job situation (e.g., Greenhalgh and Rosenblatt 1984; Probst 2003). If managers are concerned that more salient reporting of comprehensive income might adversely affect the firm's perceived performance in the future (keeping in mind this location choice generally turns out to be a long-term decision), then in our setting the relevant threat to job security is that which arises from an unfavorable future outcome.

Thus, differential retention despite poor firm performance is key to the CEO's perception of job security. Prior literature concludes that cross-sectional differences in CEOs' ability to retain their positions if faced with threatened job situations arising from unfavorable outcomes are associated with two features of CEOs' relations with the board: (1) whether the CEO also chairs the board (termed *CEO-chair duality*) and (2) the percentage of outside board members. It is not surprising that the CEO's relation with the board of directors is an important explanator of differential retention given the board's primary responsibility is hiring, monitoring, and if necessary dismissing the CEO (Mace 1986; Kaplan 1994; Hermalin and Weisbach 2003).[18]

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[17] However, recent research questions whether options really induce managers to make riskier operating and investment decisions. Ross (2004, 207) develops a model where options do not induce managers to make riskier choices, and concludes that "The common folklore that giving options to agents will make them more willing to take risks is false." In the accounting literature, Lambert et al. (1991) show that if the probability a manager's options will finish in-the-money is sufficiently high (and the vast majority of our sample's options are in-the-money), risk-averse managers have incentives to *decrease* volatility because they do not want to risk a fall in stock price that will render the options out-of-the-money. Results of the Sawers et al. (2006) laboratory experiment are consistent with the Lambert et al. (1991) prediction that managers with in-the-money options make less risky choices.

[18] We are not necessarily arguing that CEOs believe that board members were subject to limited attention that would make them more likely to overlook comprehensive income if it were reported in the statement of equity. Rather, all that is necessary is that managers were concerned that more salient performance reporting could hurt stock price, which in turn would reflect poorly on the CEO's performance as illustrated in Figure 1.

With regard to CEO-chair duality, both Lucier et al. (2004) and Desai et al. (2006) show that CEOs who also chair the board enjoy lower turnover. More on point, even after controlling for the main effect of CEO-chair duality, Goyal and Park (2002) show that the sensitivity of CEO turnover to firm performance is significantly lower with CEO-chair duality. With regard to outside board members, many studies argue that boards with more outsiders are more likely to fire CEOs if the firm experiences poor performance (e.g., Fredrickson et al. 1988; Friedman and Singh 1989; Laux 2007). In addition, Huson et al. (2001) provide empirical evidence that boards dominated by outside directors are associated with higher forced CEO turnover. More importantly, after controlling for the main effect of board independence, Weisbach (1988) demonstrates that the sensitivity of CEO turnover to poor performance is lower when the board is dominated by insiders. Collectively, this empirical evidence suggests that if a CEO faces a threatened job situation arising from an unfavorable outcome, his job is disproportionately more secure (i.e., any elevated risk of dismissal is attenuated) if he also chairs the board and if the board is dominated by insiders. Our proxy for job security encompasses these two factors, measured as of the first year the firm reports comprehensive income:

$$J\_SECURITY = CHAIRMAN + DIRECTORS \tag{2}$$

where:

$CHAIRMAN$ = 1 if the CEO also chairs the board of directors; 0 otherwise; and
$DIRECTORS$ = 1 if the percentage of outside directors on the firm's board is *smaller* than the sample median; 0 otherwise.

Higher *J_SECURITY* suggests the CEO has more job security.[19]

A limitation of our proxy for job security is that it may also reflect weaker corporate governance (Shleifer and Vishny 1997). Although weak governance has been associated with less transparent reporting in other contexts (e.g., Beasley 1996; Klein 2002; Farber 2005), we expect governance to be at most a second-order effect in our setting. It is unlikely that the board would even be involved in the comprehensive income location decision. And even if it was, Jensen's (2005) arguments suggest that the board would not necessarily have insisted on the more transparent performance reporting if it shared managers' views that this could hurt the firm's stock price. Finally, if our job security variable primarily reflects weak corporate governance, then it would be associated with the *less transparent* statement of equity location, not with the more transparent performance reporting as we hypothesize. Thus, any confound with corporate governance biases *away* from finding the hypothesized positive relation between our job security variable and the more salient performance reporting.[20]

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[19] Although prior research has shown other factors such as CEO age and firm size are associated with CEO turnover in general (e.g., Murphy 1999), we are not interested in CEO turnover in general, but rather differential retention when the CEO faces a performance-related threat to his job. Our measure of job security encompasses CEO-chair duality and board independence because both factors not only increase the likelihood of CEO retention, but are associated with differentially higher retention if a CEO faces a performance-related threat to job security.

[20] CEO job security could also reflect entrenchment (where entrenched managers take value-decreasing actions at the expense of shareholders' long-term value). But empirical evidence suggests that one of our components of job security, CEO-chair duality, does *not* primarily reflect entrenchment (Brickley et al. 1997). Even if our measure did reflect entrenchment, this would be associated with *less transparent* reporting, which would also work against finding the hypothesized relation.

Model

We use a logit model to test the determinants of firms' comprehensive income location choices:

$$\begin{aligned} \mathit{AVOID\_PERF} = {} & \beta_0 + \beta_1 \mathit{EQUITY\_INC} + \beta_2 \mathit{J\_SECURITY} + \beta_3 \mathit{MKTSEC\_D} \\ & + \beta_4 \mathit{PENSION\_D} + \beta_5 \mathit{FORCUR\_D} + \beta_6 \mathit{DISC\_QUAL} \\ & + \beta_7 \mathit{VOLATILITY} + \beta_8 \mathit{LEVERAGE} \\ & + \beta_9 \mathit{LSIZE} + \beta_{10} \mathit{E\&Y} + \varepsilon \end{aligned} \tag{3}$$

where:

AVOID_PERF = 1 if the firm avoids performance reporting by reporting comprehensive income in a statement of equity, otherwise 0;
EQUITY_INC = the sensitivity of the CEO's stock and stock option holdings to a 1 percent change in the firm's stock price, following Bergstresser and Philippon (2006), as explained above;
J_SECURITY = a measure of the CEO's job security, as explained above;
MKTSEC_D = 1 if the absolute value of unrealized gains and losses from the firm's AFS securities (hand-collected from the 10-K report and deflated by total assets), averaged over the two years prior to and the initial comprehensive income reporting year, exceeds the sample median, otherwise 0;
PENSION_D = 1 if the absolute value of unrealized gains and losses resulting from changes in the minimum pension obligation (hand-collected from the 10-K report and deflated by total assets), averaged over the two years prior to and the initial comprehensive income reporting year, exceeds the sample median, otherwise 0;
FORCUR_D = 1 if the absolute value of unrealized gains and losses from foreign currency translation (hand-collected from the 10-K report and deflated by total assets), averaged over the two years prior to and the initial comprehensive income reporting year, exceeds the sample median, otherwise 0;
DISC_QUAL = disclosure quality factor extracted from analyst following, institutional holdings, and bid-ask spreads, following Lee et al. (2006), at the end of the initial comprehensive income reporting year;
VOLATILITY = the standard deviation of comprehensive income scaled by total assets, divided by the standard deviation of net income scaled by total assets, measured over the initial comprehensive income reporting year and the prior two years;
LEVERAGE = long-term debt scaled by total assets, as of the end of the initial comprehensive income reporting year;
LSIZE = log of the market value of the firm's common shares outstanding, as of the end of the initial comprehensive income reporting year; and
E&Y = 1 if the auditor is E&Y, otherwise 0.

Hypothesis 1 predicts that the coefficient on *EQUITY_INC* will be positive if CEOs whose equity holdings are more sensitive to changes in the firm's stock price avoid performance reporting. Hypothesis 2 predicts that the coefficient on *J_SECURITY* will be negative, because we expect CEOs whose jobs are less secure to avoid the more salient performance reporting.

In addition to the primary variables used in our hypothesis tests, we control for a number of other factors. First, we control for the magnitudes of each of the other comprehensive income items by including dummy variables coded 1 if the absolute value of the item exceeds the sample median. Denoted as *MKTSEC_D*, *PENSION_D*, and *FORCUR_D*, these controls are necessary because our analysis of the comment letters on FAS No. 130 reveals that almost twice as many critics express concern about the effect of unrealized gains and losses on AFS securities as mentioned the other components. We use an above-versus below-the-median bifurcation because managers can predict the firm's *exposure* to each source of unrealized gains and losses (e.g., managers know whether the firm holds AFS securities and, thus, whether the firm is *exposed* to unrealized gains and losses), but managers are unlikely to be able to predict the *ex post* magnitude of unrealized gains and losses. Thus, while a firm's *exposure* to unrealized gains and losses may be sufficiently predictable to affect its comprehensive income reporting location choice, the magnitude of other comprehensive income items are not. As a result, there is unlikely to be a linear relation between the highly skewed raw magnitudes of other comprehensive income items and the firm's initial comprehensive income location choice.[21]

We control for disclosure quality because Lee et al. (2006) find that insurance firms with higher general disclosure quality are more likely to use performance reporting. Our proxy is similar to theirs and is based on a factor analysis of three characteristics prior research has shown are associated with higher analysts' ratings of firms' disclosure policies (i.e., higher Association for Investment Management and Research ratings): lower bid-ask spreads, higher institutional ownership, and higher analyst following (Welker 1995; Lang and Lundholm 1996; Healy et al. 1999; Bushee and Noe 2000).[22] We expect firms with lower disclosure quality are more likely to avoid performance reporting, so we expect the coefficient on *DISC_QUAL* to be negative.

We control for the relative volatility of comprehensive income to net income (*VOLATILITY*). We expect a positive coefficient on *VOLATILITY*, because firms with relatively more volatile comprehensive income may be less likely to use performance reporting. We control for leverage (*LEVERAGE*) because Graham et al. (2005) provide evidence suggesting managers of more levered firms are more concerned with smoothing earnings to minimize the perceived risk of the firm. Therefore, more levered firms may be more likely to report comprehensive income in a statement of equity. Similar to Lee et al. (2006), we also control for the log of firm size (*LSIZE*). Finally, in ancillary analysis we found that firms audited by E&Y are significantly less likely to use performance reporting than are clients of other audit firms, so, we include a control for firms audited by E&Y.

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[21] Our inferences are robust using the ranks of the other comprehensive income items. Controlling for the signed values of the other comprehensive income items does not affect our inferences, which is not surprising given their transitory nature: the average signed value of other comprehensive income is insignificantly different from zero for 93 percent of our sample firms.

[22] To maximize the power of the factor analysis and minimize estimation error in the extracted disclosure quality factor, we start with all firms listed in the 1998 Compustat file that have data for the bid-ask spread (from CRSP), analyst following (from First Call), and institutional holdings data (from CDA Spectrum). A principal factor analysis yields one common disclosure quality factor that, as expected, is a positive function of analyst following and institutional holdings and a negative function of the bid-ask spread. The factor explains about 56 percent of the total variation in the original variables. We then apply these factor coefficients to the bid-ask spread, analyst following, and institutional holdings to compute the disclosure quality factor for our sample firms.

IV. RESULTS

Descriptive Statistics

Table 2 reports descriptive statistics. The mean of *EQUITY_INC* shows that, on average, a 1 percent change in the firm's stock price spurs a 29 percent change in the CEO's total annual compensation from the firm. Our sample S&P 500 firms are large, with a median (mean) market value of equity of \$7.9 (\$20.2) billion.[23]

TABLE 2
Descriptive Statistics for Independent Variables

Variable	Mean	Std. Dev.	Lower Quartile	Median	Upper Quartile
EQUITY_INC	0.292	0.243	0.117	0.209	0.386
J_SECURITY	1.291	0.613	1	1	2
MKTSEC_D	0.500	0.501	0	0.500	1
PENSION_D	0.386	0.487	0	0	1
FORCUR_D	0.500	0.501	0	0.500	1
DISC_QUAL	1.700	0.583	1.260	1.660	2.085
VOLATILITY	1.396	1.272	0.944	1.028	1.302
LEVERAGE	0.216	0.143	0.110	0.199	0.317
LSIZE	9.047	1.277	8.233	8.979	9.818
E&Y	0.248	0.432	0	0	0
Market value of equity (in \$ millions)	20,243	34,990	3,764	7,931	18,366

EQUITY_INC is the sensitivity of the CEO's stock and stock option holdings to a 1 percent change in the firm's stock price, following Bergstresser and Philippon (2006). *J_SECURITY* is the sum of two indicator variables. The first equals 1 if the CEO also chairs the board of directors, and 0 otherwise. The second equals 1 if the percentage of independent directors on the firm's board is smaller than the sample median, and 0 otherwise. *MKTSEC_D* equals 1 if the average absolute value of the unrealized gains and losses from the firm's available-for-sale securities (hand-collected from the firm's 10-K report and then scaled by total assets), averaged over the three years starting two years before the initial comprehensive income reporting year, exceeds the sample median, and 0 otherwise. *PENSION_D* equals 1 if the average absolute value of the unrealized gains and losses resulting from changes in the minimum pension obligation (hand-collected from the firm's 10-K report and then scaled by total assets), averaged over the three years starting two years before the initial comprehensive income reporting year, exceeds the sample median, and 0 otherwise. *FORCUR_D* equals 1 if the average absolute value of unrealized gains and losses from foreign currency translation (hand-collected from the firm's 10-K report and then scaled by total assets), averaged over the three years starting two years before the initial comprehensive income reporting year, exceeds the sample median, and 0 otherwise. *DISC_QUAL* is the disclosure quality factor extracted following Lee et al. (2006), where higher values indicate higher quality disclosure. *VOLATILITY* is the standard deviation of comprehensive income scaled by total assets, divided by the standard deviation of net income scaled by total assets, measured over the initial comprehensive income reporting year and the prior two years. *LEVERAGE* is long-term debt scaled by total assets. *LSIZE* is the log of the market value of the firm's common shares outstanding. *E&Y* equals 1 if the firm's auditor is *E&Y*, and 0 otherwise.

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[23] Our mean *EQUITY_INC* (0.29) is a bit higher than Bergstresser and Philippon's (2006, 517) mean of 0.24. This is not surprising given the increase in equity incentives during the 1990s and our sample starts in 1998 whereas theirs starts in 1994. Also, for *MKTSEC_D* and *FORCUR_D*, exactly half of the observations are coded as zero because they are in the lower half of the distribution, and the other half coded as one because they are in the upper half, so the median (and mean) values of these variables are 0.5 by construction. For *PENSION_D*, only 38.6 percent of the firms report a pension component, so more than half of the observations are coded as zero. For continuous variables, all analyses are conducted after winsorizing at 1 percent and 99 percent.

Table 3 reports correlations among the independent variables. CEO equity incentives (*EQUITY_INC*) are greater for firms whose CEOs have more job security. This is not surprising because Morse et al. (2007) find that equity compensation is increasing in the influence of the CEO. Our logit analyses include both *EQUITY_INC* and *J_SECURITY* so we can identify the incremental effect of each variable after controlling for the other.

Equity incentives are also associated with several control variables: equity incentives are greater in larger firms, and equity incentives are also associated with the *MKTSEC_D* and *PENSION_D* components of other comprehensive income. As in prior research, *DISC_QUAL* is higher for larger firms (Lang and Lundholm 1993; Kasznik and Lev 1995).

Logit Results

Table 4 displays the results of estimating the logit model in Equation (3) with and without controlling for industry (Bowen et al. 1995). Specifically, we add a dummy variable for each major economic sector identified by the Global Industry Classification Standards (GICS) codes because Bhojraj et al. (2003) show that they generate more homogenous industry groups than SIC codes or the Fama and French (1997) algorithm. (Our results are unaffected when we use one-digit SIC classifications.) The benchmark industry (whose effect is impounded in the intercept) is *Energy*. Significance levels are based on robust standard errors correcting for heteroscedasticity and dependence across firms within the same industry (Froot 1989; Wooldridge 2002).

The results support our hypothesis that firms whose CEOs have higher equity-based incentives are more likely to avoid performance reporting ($p \leq 0.01$), whether or not we control for industry. Table 4 also shows that CEOs who enjoy greater job security are less likely to avoid performance reporting, consistent with our second hypothesis ($p \leq 0.03$). These inferences are robust to alternative specifications of equity incentives and job security.[24]

We calculate the percentage change in odds for *EQUITY_INC* and *J_SECURITY* to assess the effect of a change in the independent variable on the odds of reporting comprehensive income in a statement of equity, holding all other independent variables at their means.[25] We find that a one-standard-deviation increase in *EQUITY_INC* increases the odds the manager avoids performance reporting by 64.7 percent. A one-unit increase in *J_SECURITY* (which ranges from 0 to 2) reduces the odds the manager avoids performance reporting by 28 percent. Thus, differences in our determinants of comprehensive income location choice are associated with what appear to be meaningful reporting differences.

Table 4 also shows that firms with large absolute unrealized gains and losses on AFS securities (i.e., *MKTSEC_D* = 1) are less likely to use performance reporting ($p < 0.04$). Since 60 percent of our sample firms have unrealized gains and losses on AFS securities, whereas 40 percent do not, our result suggests that firms exposed to unrealized gains and losses from AFS securities tend not to use performance reporting.

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[24] Our primary analysis is based on Bergstresser and Philippon's (2006) measure of equity incentives because we believe the portion of the CEO's compensation from the firm stemming from a 1 percent change in stock price best captures the power of equity incentives. However, our inferences are robust using either the natural logarithm of *ONEPCT* following Burns and Kedia (2006), or a measure of the manager's stake in the firm, using the CEO's total stock and option holdings deflated by the firm's total outstanding shares, following Cheng and Warfield (2005). With respect to job security, our main analysis considers independent and gray directors to be outside directors. Our inferences are unaffected if we classify only independent directors as outside directors.

[25] For continuous variables, the percentage change in odds is $100\ [\exp(\beta_i \times \delta_i) - 1]$, where β_i is the estimated coefficient for variable *i* and δ_i is the sample standard deviation of variable *i*. For discrete variables, the percentage change in odds is $100\ [\exp(\beta_i) - 1]$.

TABLE 3
Correlations among Independent Variables

	EQUITY_INC	*J_SECURITY*	*MKTSEC_D*	*PENSION_D*	*FORCUR_D*	*DISC_QUAL*	*VOLATILITY*	*LEVERAGE*	*LSIZE*	*E&Y*
EQUITY_INC		0.193 (< 0.001)	0.143 (0.003)	−0.195 (< 0.001)	0.048 (0.319)	0.126 (0.008)	−0.061 (0.198)	−0.201 (< 0.001)	0.351 (< 0.001)	0.082 (0.084)
J_SECURITY	0.167 (< 0.001)		−0.052 (0.277)	0.004 (0.931)	−0.052 (0.277)	−0.057 (0.229)	0.019 (0.686)	0.001 (0.980)	0.041 (0.390)	−0.023 (0.626)
MKTSEC_D	0.156 (0.001)	−0.043 (0.372)		−0.140 (0.003)	−0.145 (0.002)	0.109 (0.022)	0.194 (< 0.001)	−0.252 (< 0.001)	0.136 (0.004)	0.132 (0.006)
PENSION_D	−0.192 (< 0.001)	−0.002 (0.974)	−0.140 (0.003)		0.112 (0.019)	−0.020 (0.676)	−0.137 (0.004)	0.108 (0.023)	−0.094 (0.049)	−0.088 (0.066)
FORCUR_D	0.066 (0.165)	−0.058 (0.224)	−0.145 (0.002)	0.112 (0.019)		−0.014 (0.765)	−0.097 (0.042)	−0.020 (0.670)	−0.026 (0.587)	−0.090 (0.061)
DISC_QUAL	0.190 (< 0.001)	−0.045 (0.346)	0.111 (0.020)	−0.032 (0.505)	−0.023 (0.625)		−0.026 (0.586)	0.018 (0.704)	0.582 (< 0.001)	−0.052 (0.273)
VOLATILITY	−0.094 (0.048)	−0.016 (0.731)	0.140 (0.003)	−0.076 (0.109)	−0.059 (0.219)	−0.034 (0.474)		−0.182 (< 0.001)	0.110 (0.021)	0.039 (0.413)
LEVERAGE	−0.239 (< 0.001)	−0.009 (0.852)	−0.265 (< 0.001)	0.134 (0.005)	−0.006 (0.900)	0.019 (0.697)	−0.090 (0.058)		−0.180 (0.000)	−0.070 (0.143)
LSIZE	0.446 (< 0.001)	0.040 (0.400)	0.137 (0.004)	−0.107 (0.025)	−0.035 (0.469)	0.568 (< 0.001)	0.046 (0.341)	−0.186 (< 0.001)		−0.078 (0.103)
E&Y	0.091 (0.056)	−0.026 (0.587)	0.132 (0.006)	−0.088 (0.066)	−0.090 (0.061)	−0.071 (0.136)	−0.015 (0.751)	−0.065 (0.171)	−0.080 (0.094)	

Pearson correlations appear above the diagonal, and Spearman correlations appear below the diagonal. Two-tailed p-values are in parentheses.
Table 2 defines the variables.

TABLE 4
Logit Analysis of Comprehensive Income Reporting Location Choice
(Dependent variable = 1 if the firm avoids performance reporting, and 0 otherwise)

Variable (n = 440)	Predicted Sign	Without Industry Controls (1)	With Industry Controls (2)
EQUITY_INC	+	2.056 (0.001)	1.538 (0.004)
J_SECURITY	–	−0.329 (0.026)	−0.334 (0.031)
MKTSEC_D	?	0.375 (0.036)	0.503 (0.026)
PENSION_D	?	−0.011 (0.951)	0.041 (0.802)
FORCUR_D	?	0.443 (0.101)	0.241 (0.341)
DISC_QUAL	–	−0.403 (0.112)	−0.224 (0.287)
VOLATILITY	+	0.132 (0.044)	0.154 (0.022)
LEVERAGE	?	0.584 (0.619)	0.649 (0.586)
LSIZE	?	0.044 (0.784)	0.011 (0.957)
E&Y	?	0.969 (0.003)	0.949 (0.003)
Consumer Discretionary	?		1.535 (0.000)
Consumer Staples	?		2.206 (0.000)
Financials	?		0.448 (0.114)
Health Care	?		0.485 (0.013)
Industrials	?		0.827 (0.020)
Information Technology	?		1.01 (0.000)
Materials	?		0.677 (0.022)
Telecommunication Service	?		1.876 (0.000)
Utilities	?		0.058 (0.797)
Intercept	?	0.756 (0.631)	0.059 (0.977)
McKelvey and Zavoina's (1975) R^2		0.158	0.216

Independent variables are defined in Table 2, except that industry dummies equal 1 if a firm is in the given industry. In column (2), energy industry is the benchmark industry whose effect is impounded in the intercept. p-values (in parentheses) are based on standard errors adjusted for heteroscedasticity and for correlation across firms within a given industry. p-values are one-tailed for variables with predictions, and two-tailed for variables without predictions.

With respect to the remaining control variables, the coefficient on *DISC_QUAL* is negative but not significant at conventional levels.[26] The coefficient on *VOLATILITY* is positive ($p < 0.05$), so firms with relatively more volatile comprehensive income avoid performance reporting.[27] Firms audited by E&Y tend to avoid performance reporting. The coefficients on the pension and foreign currency components, leverage, and firm size are not significant. Many of the industry dummy variables are significant, indicating systematic across-industry differences in reporting location. (Thus, in our logit analysis we use robust standard errors that correct for correlations across firms in the same industry when we do not include industry controls.)

Analysis of Firms Changing their Comprehensive Income Reporting Location

To help ensure our inferences are not simply an artifact of some unidentified correlated omitted variable, we examine the small set of firms that changed their comprehensive income reporting location between their initial reporting year and 2001. We start with the 14 firms in the S&P 500 (as of December 1998) omitted from our primary sample because they changed their location. We examined the 133 firms included in the October 2002 S&P 500 that were not in our original sample, and identified six firms that changed their reporting location between 1998 and 2001. This yields 20 firms that changed their comprehensive income reporting location.[28]

Table 5, Panel A reports the changes in our explanatory variables between the year the firm first reported comprehensive income and the year the firm changed its reporting location. We denote these "change" variables with the prefix Δ. For the 13 firms that changed away from performance reporting, *EQUITY_INC increased*, while *J_SECURITY decreased*, on average. In stark contrast, for the seven firms that switched to performance reporting, *EQUITY_INC decreased* and *J_SECURITY increased*. The difference across reporting locations for changes in *EQUITY_INC* and *J_SECURITY* are significant ($p = 0.07$ and $p = 0.04$, respectively), consistent with increasingly powerful equity incentives and decreased job security reducing the likelihood of performance reporting. *VOLATILITY* increased more for firms that changed to the statement of equity ($p = 0.08$), but none of the other explanatory variables changed significantly.

Panel B of Table 5 reports the results of a logit analysis on the change firms, where the dependent variable equals 1 for firms that switch from performance reporting to the

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[26] Disclosure quality is significantly correlated with other explanatory variables. When we repeat the logit analysis including only equity incentives, job security, and disclosure quality as independent variables, the coefficient on disclosure quality is negative and significant as expected ($p \leq 0.05$), and the coefficients on equity incentives and job security remain significant at $p \leq 0.02$.

[27] Lee et al. (2006) report a positive but insignificant relation between avoiding performance reporting and their volatility measure. A likely explanation for the stronger significance level in our analysis is that our sample is five and a half times larger than their sample. There are also differences in the specifications of the relative volatility measures. Lee et al. (2006) include some estimated comprehensive income data before FAS No. 130 and do not scale their income measures, whereas we use as-reported comprehensive income data and we scale the time-series of comprehensive income and net income by assets in order to control for growth.

[28] Restricting our analysis to the 14 change firms deleted from our original sample yields similar inferences. We focus on changes in comprehensive income reporting between 1998 and 2001 because the Sarbanes-Oxley Act that took effect in 2002 affected firms' financial reporting behavior (e.g., Lobo and Zhou 2006). We found that 11 of our sample firms changed comprehensive income reporting location between 2002 and 2007. In additional analysis, we found that ten stopped using performance reporting. The only firm that changed to the more transparent performance reporting did so in the same year the SEC investigated it for questionable accounting practices. For the ten firms that stopped using performance reporting, their CEOs' equity incentives increased significantly more than the increase in equity incentives for the other (no-change) firms in our sample ($p = 0.03$). There were not, however, significant differences in the changes in *J_SECURITY* across these 11 change firms and no-change firms.

TABLE 5
Analysis of the 20 Firms that Changed Comprehensive Income Reporting Location

Panel A: Univariate Tests of Differences between Firms Changing Reporting Location

Variable	(1) Change to Statement of Equity (n = 13)	(2) Change to Performance Reporting (n = 7)	Expected Difference: (1) − (2)	Actual Difference: (1) − (2)	p-value of Difference
Δ*EQUITY_INC*	0.059	−0.141	+	0.200	0.073
Δ*J_SECURITY*	−0.539	0.429	−	−0.967	0.039
Δ*MKTSEC_D*	0.615	0.286	?	0.330	0.179
Δ*PENSION_D*	0.231	0.143	?	0.088	0.647
Δ*FORCUR_D*	0.308	0.429	?	−0.121	0.627
Δ*DISC_QUAL*	0.068	−0.019	−	0.087	0.707
Δ*VOLATILITY*	1.330	−0.005	+	1.335	0.081
Δ*LEVERAGE*	0.320	0.259	?	0.061	0.473
Δ*LSIZE*	8.957	8.723	?	0.233	0.672
Δ*E&Y*	0	0	?	0	NA

Panel B: Logit Analysis of Decision to Change Comprehensive Income Reporting Location

Dependent Variable = 1 if change is to a statement of equity and 0 if change is to performance reporting	Predicted Sign	Coefficient (p-value)
Δ*EQUITY_INC*	+	13.462 (0.018)
Δ*J_SECURITY*	−	−2.873 (0.015)

McKelvey and Zavoina's (1975) $R^2 = 0.821$

Δ*EQUITY_INC* is the difference in *EQUITY_INC* between the year the firm changed its comprehensive income reporting location and the initial year it reported comprehensive income. Similarly, Δ*J_SECURITY*, Δ*MKTSEC_D*, Δ*PENSION_D*, Δ*FORCUR_D*, Δ*DISC_QUAL*, Δ*VOLATILITY*, Δ*LEVERAGE*, Δ*LSIZE*, and Δ*E&Y* are the changes in the original variables (defined in Table 2) between the year the firm changed its comprehensive income reporting location and the initial year the firm reported comprehensive income. In Panel A, reported p-values are based on two-sample t-tests and are one-tailed for variables with predictions and two-tailed for variables without predictions. Reported p-values (in parentheses) in Panel B are one-tailed, and are based on standard errors adjusted for heteroscedasticity and for correlation across firms within a given industry.

statement of equity location, otherwise 0. We include only the change version of our two key variables, Δ*EQUITY_INC* and Δ*J_SECURITY*, because we have so few degrees of freedom (adding a control for Δ*VOLATILITY* does not affect our inferences). Consistent with the univariate change analysis, the coefficient is positive on Δ*EQUITY_INC* ($p = 0.018$) and negative on Δ*J_SECURITY* ($p = 0.015$). CEOs with increasingly powerful equity incentives and decreasing job security are more likely to switch away from performance reporting. The consistency of our changes and levels analyses suggests our conclusions are not attributable to some unidentified firm-specific effect or correlated omitted variable. The results of our change analysis further indicate that managers continue to act as if reporting location matters.

Additional Analyses

Our primary sample focused on S&P 500 firms, because of their economic importance and better data availability. Evidence that smaller firms' investors may be more subject to limited attention (Huddart et al. 2009) suggests managers of smaller firms may be more concerned about investors' response to salient reporting of comprehensive income. To assess whether our inferences hold for smaller firms, we draw a random sample of 300 firms that have compensation data on ExecuComp, but are not in the S&P 500. Of these 300 firms, 181 report comprehensive income different from net income in the year FAS No. 130 is effective, and have the necessary data to run our primary model.

The results of the logit analysis on this sample appear in Table 6. Consistent with the primary results in Table 4, the likelihood a firm avoids performance reporting is increasing in the power of the CEO's equity incentives ($p \leq 0.058$), and decreasing in our measure of job security ($p < 0.05$). Consistent with managers of smaller firms being more concerned about how the location of comprehensive income might affect stock prices and job security, the coefficients on this sample of smaller firms are larger in absolute magnitude than the coefficients on our sample of S&P 500 firms (reported in Table 4), although the differences in the coefficients are not statistically significant at conventional levels, probably due to the smaller number of observations in this additional sample. Nonetheless, these results demonstrate that our inferences generalize to the population of firms in ExecuComp.

Our primary analysis uses Equation (3) for parsimony, but our results are robust to controlling for a number of other variables. We repeat the analysis including a variable indicating whether the firm uses an industry-specialist auditor, because Lee et al. (2006) find that insurance firms using an industry specialist are more likely to use performance reporting. We also include an indicator variable to identify firms that have an independent audit committee, and another indicator variable to identify firms that have at least one audit committee member who is a financial expert (following DeFond et al. [2005]). None of these indicators are significant and their inclusion does not affect our inferences. Similarly, including other variables sometimes associated with accounting choice, such as a proxy for the firm's financing needs (issuance of debt and equity, scaled by total assets) and the natural log of the market-to-book ratio (to control for growth opportunities) does not affect our inferences, and the coefficients on these control variables are not significant. We also reestimate our logit model after eliminating firms in the financial industry (which generally have more AFS securities) or firms in high-tech industries (which tend to have larger equity incentives). Our primary inferences are unaffected.[29]

Our analysis thus far combines the two forms of performance reporting: (1) the combined statement of net income and comprehensive income, and (2) the separate statement of comprehensive income (as did Lee et al. [2006]), effectively assuming both presentations are equally salient. One could argue the combined statement is the most salient, the separate statement of comprehensive income is in the middle, and the statement of equity presentation is least salient. We estimate an ordered logit model with the three aforementioned categories. Results are similar to those based on the dichotomous classification—both *EQUITY_INC* and *J_SECURITY* remain significant at $p \leq 0.03$ without industry controls and $p \leq 0.055$ with industry controls. The Brant (1990) proportional odds test reveals that

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[29] We would also like to control for cherry-picking similar to Lee et al. (2006), but we cannot because only about one-quarter of our sample firms disclose realized gains and losses on AFS securities whereas FAS No. 60 (paragraph 50; FASB 1982) uniquely requires all property-liability insurers to separately disclose them. Although we cannot directly capture *ex post* cherry-picking, our *MKTSEC_D* variable captures the *ex ante* opportunity to cherry-pick (which is a necessary, though not sufficient condition for actual cherry-picking).

TABLE 6
Logit Analysis of Comprehensive Income Reporting Location Choice for a Random Sample of 181 Non-S&P 500 Firms
(Dependent variable = 1 if the firm avoids performance reporting, and 0 otherwise)

Variable (n = 181)	Predicted Sign	Without Industry Controls (1)	With Industry Controls (2)
EQUITY_INC	+	3.098	2.74
		(0.004)	(0.058)
J_SECURITY	−	−0.602	−0.835
		(0.044)	(0.035)
MKTSEC_D	?	−0.02	0.151
		(0.954)	(0.674)
PENSION_D	?	0.093	−0.143
		(0.801)	(0.726)
FORCUR_D	?	0.003	−0.002
		(0.992)	(0.997)
DISC_QUAL	−	0.502	0.529
		(0.911)	(0.864)
VOLATILITY	+	−0.056	−0.071
		(0.922)	(0.907)
LEVERAGE	?	−4.161	−4.786
		0.000	(0.003)
LSIZE	?	−0.609	−0.608
		(0.011)	(0.083)
E&Y	?	0.698	0.575
		(0.346)	(0.430)
Consumer Discretionary	?		1.73
			(0.017)
Financials	?		−0.774
			(0.363)
Health Care	?		−1.593
			(0.053)
Industrials	?		−0.375
			(0.614)
Information Technology	?		0.351
			(0.740)
Materials	?		0.139
			(0.835)
Utilities	?		−1.324
			(0.148)
Intercept	?	6.449	7.131
		(0.000)	(0.008)
McKelvey and Zavoina's (1975) R^2		0.276	0.407

Independent variables are defined in Table 2, except that industry dummies equal 1 if a firm is in the given industry. In column (2), energy industry is the benchmark industry whose effect is impounded in the intercept. p-values (in parentheses) are based on standard errors adjusted for heteroscedasticity and for correlation across firms within a given industry. p-values are one-tailed for variables with predictions, and two-tailed for variables without predictions.

the coefficients in a logit model explaining (1) the statement of equity versus the separate statement of comprehensive income choice are not significantly different from those in a logit model explaining (2) the separate statement of comprehensive income versus a single combined statement of net income and comprehensive income. While this result is consistent with three ordered categories, our primary analysis relies on the dichotomous specification because: (1) the FASB considers both the combined statement format and the separate statement of comprehensive income as *performance reporting* since both formats are performance statements that end with comprehensive income as the bottom line figure, and consequently, and (2) prior research (e.g., Lee et al. 2006; Chambers et al. 2007) combines the two performance statement formats.

V. CONCLUSIONS

Policymakers prefer that firms report comprehensive income in a performance statement, which they consider as more transparent, rather than in the statement of equity. As this preference simply concerns a reporting *location* choice, one would expect managers to acquiesce because they place a great deal of importance on developing a reputation for transparent reporting (Graham et al. 2005, 54). However, over 80 percent of our sample S&P 500 firms do *not* follow policymakers' preference, and instead report comprehensive income in the statement of equity. Our study provides new insight into why so many firms do not follow policymakers' preference for reporting comprehensive income in the more salient performance statement.

Because traditional theories of accounting choice based on existing contracting incentives cannot explain a choice that is purely one of *location,* we draw on prior theoretical, survey-based, and behavioral research to identify new factors that we expect to affect this choice. This literature suggests that at the time managers made the initial comprehensive income reporting choice, they were concerned that reporting comprehensive income—which is typically more volatile than net income—in a performance statement could increase the perceived volatility of the firm's performance (e.g., Hirshleifer and Teoh 2003; Yen et al. 2007; Maines and McDaniel 2000). Surveys and behavioral research show that managers *expect* that users who perceive the firm's performance as more volatile will place a lower value on the stock and will assess the manager as less competent (Graham et al. 2005; Maines and McDaniel 2000).

Our empirical results support our hypotheses that managers who avoid performance reporting are those with more powerful equity-based incentives that will be devalued by a lower stock price and those with less job security who have more to lose from a poor performance evaluation. The magnitudes of these effects suggest managers' unique personal equity-based incentives and job security concerns lead to meaningful differences in reporting behavior. Analysis of the small set of firms that change their comprehensive income reporting location further supports our inferences: CEOs with increasingly powerful equity incentives and decreasing job security are more likely to switch away from performance reporting. Our inferences also generalize to a random sample of smaller firms not in the S&P 500.

Our evidence suggests that at the time managers made the initial comprehensive income reporting location decision, they believed performance reporting would be more salient—consistent with views of the FASB and IASB and evidence from laboratory studies (e.g., Hirst and Hopkins 1998; Maines and McDaniel 2000; Hunton et al. 2006). Our change analysis suggests managers continue to believe reporting location matters. Our evidence on who refuses to follow policymakers' stated preferences on what seems to be an inconsequential reporting location choice is relevant as policymakers consider requiring all firms to use performance reporting (IASB 2008; FASB 2008).

Our result that managers with less job security on average make reporting choices that reduce transparency is of interest in its own right, and also helps fill the void that Graham et al. (2005) identify when they point out the dearth of evidence on how managers' career concerns affect their financial reporting choices. Our results also extend prior research showing that equity-based compensation increases incentives for earnings management (e.g., Cheng and Warfield 2005; Bergstresser and Philippon 2006) by showing that equity incentives affect another transparency-related accounting choice: whether to disclose comprehensive income in a more or less salient location.

Finally, in comment letters on the initial FAS No. 130 proposal, managers expressed concern that performance reporting would lead stakeholders to view firms' performance as more volatile than warranted by the actual economics. Our evidence suggests that the concerns expressed in the comment letters are real (as distinct from excuses).

Future research can complement our study of the determinants of comprehensive income reporting location by examining the *consequences* of the location choice. Specifically, research could further probe investors' actual reactions to comprehensive income disclosures, in order to assess the *ex post* validity of managers' *ex ante* concerns. Does performance reporting increase investors' assessments of the firm's risk and hurt the firm's stock price and the manager's performance evaluation in real markets? It would also be interesting to extend Chambers et al. (2007) to further explore whether investors react differently to comprehensive income reported in a performance statement versus a statement of equity. Such a valuation study should consider modeling the determinants of the location choice to control for self-selection biases, and incorporating the component of other comprehensive income that represents the reclassification of realized gains and losses.

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Client Importance, Institutional Improvements, and Audit Quality in China: An Office and Individual Auditor Level Analysis

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ABSTRACT: This study examines how the legal and regulatory changes in China affect the relationship between client economic importance and audit quality. At the individual auditor level, we find that the propensity to issue modified audit opinions (MAOs) is *negatively* correlated with client importance from 1995 to 2000. However, from 2001 to 2004, when the institutional environment became more investor-friendly, the propensity to issue MAOs is *positively* associated with client importance. These findings are corroborated by an analysis of regulatory sanctions. Although client importance measured at the office level is also negatively related to the propensity for MAOs from 1995 to 2000 without controlling for the auditor-level client importance, this result is sensitive to model specification and sample composition. Our results suggest that (1) institutional improvements prompt auditors to prioritize the costs of compromising quality over the economic benefits gained from important clients; and (2) the impact of client importance on audit decisions appears to be different at the individual auditor and office levels.

Keywords: *client importance; audit quality; litigation; audit-reporting decision.*

Data Availability: *The data are publicly available from the sources identified in the paper.*

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I. INTRODUCTION

In this study, we examine the relationships among client economic importance, institutional improvements, and audit quality in China at the audit office and individual auditor levels. At issue is whether the impact of client economic importance on audit quality depends on the institutional environment and the level of analysis. It has long been suspected that the financial interests auditors have in their clients can adversely affect their independence (Mautz and Sharaf 1961; AICPA 1978). The Enron scandal, together with the collapse of Arthur Andersen, has pushed the auditor independence issue even further to the forefront. Nonetheless, the empirical evidence has been less than consistent (e.g., Frankel et al. 2002; Ashbaugh et al. 2003; Chung and Kallapur 2003). Previous studies, predominantly based on developed markets with better investor protection, normally attribute the lack of evidence of the impact of client importance to auditor concerns about litigation and/or reputation (e.g., Reynolds and Francis 2000). We directly test this conjecture in the Chinese stock market. As the institutional environment for investor protection is generally perceived to be weaker in China than in more mature markets, we should be more likely to observe the theorized negative impact of client dependence on auditor quality (DeAngelo 1981). More importantly, the substantial institutional improvements in China during our sample period provide us with a natural "laboratory" in which to examine whether auditor attitudes toward economically important clients change when Chinese institutions become more investor friendly.

Extant studies on client importance have been conducted either at the audit firm or office levels. Consistent with the argument that the audit office is a more appropriate level of analysis than the audit firm (Francis et al. 1999; Reynolds and Francis 2000), DeFond and Francis (2005) suggest that the level of analysis be pushed down further to an individual partner level for a better understanding of auditor behavior. As the engagement auditor is required to sign the audit report in China, we are able to conduct our analysis at the individual auditor level and compare it to the analysis at the office level.

Based on a sample of 8,917 firm-year observations from 1995 to 2004, we find that client importance as measured at the individual auditor level impaired audit quality in China for the period from 1995 to 2000 (the pre-2001 period) when the institutions for investor protection were relatively weak. Specifically, after controlling for other factors that affect modified audit opinions (MAOs), we find that the probability of issuing MAOs was significantly *lower* for clients that were more important to individual auditors. However, after 2001 (the post-2001 period), when the laws and regulations surrounding audit failures became harsher, the probability of receiving an MAO became *higher* for clients that were more important to individual auditors. Controlling for the individual auditor level impact, we find that the client importance measured at the office level is not significant in explaining the propensity of auditors to issue MAOs before or after 2001. Although the office-level measure is also significantly negatively related to MAO issuance in the model *without* controlling for the auditor-level measure before 2001, this result is mainly caused by a portion of observations in which the client importance to the office is similar to that to the individual auditors. Subject to the caveat of the possible multicollinearity problem caused by the correlation between the two measures, the effect of client importance appears to be stronger at the auditor level than at the office level.

Our findings are robust to potential econometric and measurement issues. We also corroborate our main findings by analyzing the consequences of issuing inappropriate audit reports. We show that for the fiscal years during the pre-2001 period, the probability of audit failure cases sanctioned by the Chinese regulator was *positively* related to client importance at the individual auditor level. This implies that individual auditors are more likely to compromise their objectivity in auditing their key clients. However, the probability of sanctioned audit failures involving economically important clients *decreases* significantly during the post-2001 period. Again, we find no such evidence at the office level.

Overall, our findings suggest that auditors in China are more likely to compromise audit quality for economically important clients when the institutions for investor protection are weak. Nevertheless, the institutional improvements in China help to mitigate that compromise to the extent that auditors' concerns about litigation risks and regulatory sanctions dominate their economic incentives. Previous studies conjecture that such concerns affect the relation between client importance and audit quality, suggesting that the analysis of client importance at the individual auditor level would be the most pertinent. We contribute to the literature by providing evidence in line with this conjecture. In addition, our study contributes to a growing body of literature on the impact of institutions on auditing practices across countries (Fan and Wong 2005; Choi and Wong 2007; Francis and Wang 2008). There are serious methodological limitations when using cross-country data, such as small sample sizes, endogeneity, and omitted variables (Miller 2004; Gul 2006). By providing time-series evidence of the impact of institutional changes on auditor behavior in a single country, our research complements cross-country studies.

Section II presents background information and discusses our research questions. The research methods and empirical results follow in Section III. We check the robustness of our findings in Section IV, and conduct additional analyses in Section V. Finally, we summarize and discuss our findings in Section VI.

II. BACKGROUND AND HYPOTHESIS DEVELOPMENT

Extant Literature

Because of significant client-specific start-up costs, incumbent auditors have cost advantages over competitors and earn quasi rents (i.e., audit fees in excess of audit costs) in subsequent audits. Hence, incumbent auditors are not expected to be perfectly independent of their clients, as the latter can impose real costs on the former by terminating the bilateral relationship (DeAngelo 1981). *Ceteris paribus*, the larger the client in an auditor's portfolio, the stronger should be the incentive that the auditor has to retain that client and thus possibly compromise audit quality.

The empirical evidence is, however, mixed. Studies based on investor perception provide more supportive evidence. Both Krishnan et al. (2005) and Francis and Ke (2006) find lower earnings response coefficients for firms paying higher nonaudit fees, and Khurana and Raman (2006) show that higher audit fees are associated with a higher cost of equity. Although these findings can be considered to reflect investors' concerns about the negative impact of client dependence, we cannot necessarily conclude that there is an actual adverse effect. On the contrary, there is more evidence suggesting no negative impact of client importance on auditor quality, as measured by earnings management (Chung and Kallapur 2003), earnings restatement (Kinney et al. 2004), or modified audit opinions (Craswell et al. 2002).[1] Reynolds and Francis (2000) and Gaver and Paterson (2007) find that U.S. Big 5 auditors are even more conservative in auditing clients that are more important to the local audit offices, which is consistent with the argument that audit failures involving larger clients are more likely to result in lawsuits, such that auditors exercise greater care in auditing such clients (Stice 1991; Lys and Watts 1994).

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[1] Although Frankel et al. (2002) show a positive relation between nonaudit service purchases and earnings management, their findings and methods have been questioned in many follow-up studies (e.g., Ashbaugh et al. 2003; Chung and Kallapur 2003; Larcker and Richardson 2004).

More research is needed to untangle the relationship between client dependence and audit quality. An unanswered question is whether the institutional environment for investor protection is the reason for the failure to identify the negative impact of client dependence that has been conjectured in the literature (Reynolds and Francis 2000; Craswell et al. 2002; DeFond et al. 2002). China provides an interesting setting for such a test.

The Pre-2001 Institutional Environment in China

An important feature of China's audit market is its fierce competition. Unlike developed economies in which the Big 4 auditors audit the majority of listed companies, concentration in the Chinese audit market for listed companies is rather low. In 2001, the concentration ratio for the Top 4 and Top 8 auditors was 30.32 percent and 44.70 percent, respectively (Xia and Lin 2003), and the average market share of the Big 5 (now the Big 4) auditors in the statutory audit market between 1995 and 2003 was 26 percent (Chen et al. 2007). In addition, the number of listed companies in China is small relative to that of the CPA firms qualified to audit them. At the end of 2006, there were 73 audit firms qualified to audit approximately 1,400 listed companies, which means that one qualified firm had less than 20 listed clients on average. Such a buyer's market is likely to afford clients more bargaining power and impose pressure on auditors fighting for their slice of the cake (Chen et al. 2007). Moreover, there is a lack of demand for quality auditing. To avoid drawing regulatory attention, managers of Chinese-listed companies are averse to modified audit opinions. DeFond et al. (1999) show that Chinese audit firms with a greater propensity to issue modified reports tended to lose market share during the 1994–1996 period. Fierce competition, together with a lack of demand for high-quality audit services, means that the problem of dependence on important client could be more obtrusive in China.

Chen (2003) argues that there was no legal infrastructure in earlier years to support capital market development in China, as private securities litigation against accounting fraud and market manipulation was not significant until 2001. As a result, compared to those in developed markets such as the U.S., the regulatory enforcement and litigation risk faced by Chinese auditors were relatively low in the 1990s. The penalties for auditor violation of the audit regulations remained mostly at the administrative level, taking the form of public reprimands, warnings, fines, and suspension of licenses to practice. The number of CPA firms brought to court for civil or criminal liability was negligible (Pistor and Xu 2005). The legal framework for CPA civil liability was fundamentally problematic: the civil law did not give consistent definitions of auditor liability, was ambiguous in its compensation procedures, and explicitly forbade class action litigation (Li and He 2000).

Starting in 1996, the Chinese government took steps to improve the institutional environment of the stock market. These steps included the disaffiliation program,[2] the development of accounting and auditing standards, and legislative measures such as the Securities Law (effective on July 1, 1999). The impact of these improvements was, however, not much felt until 2001, after the massive exposure of accounting frauds and audit failures caused a public outcry.

Changes in the Institutional Environment around 2001

The large-scale exposure of fraud was triggered by the scandal of ZhengBaiWen (Stock Code: 600898) in 2000, followed by two cases involving MonkeyKing (Stock Code:

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[2] Chinese CPA firms were originally part of or affiliated with governmental agencies. In 1997, the China Securities Regulatory Commission (CSRC) initiated a disaffiliation program that transformed these firms into partnership or limited liability companies.

000535) and the star company Yorkpoint (Stock Code: 000008), and ending with the Chinese "Enron," YinGuangXia (Stock Code: 000557) in August 2001. All of these scandals involved earnings manipulation or fraudulent financial reporting, but none of the companies received any modified audit opinions from their auditors, including ZhongTianQin, the auditor of YinGuangXia, then the largest audit firm in China. These scandals contributed to a market crash later in 2001, which in turn led to a fight for compensation by affected investors.

Consistent with Coffee's (2001) "crash-then-law" interpretation of legal developments, these events provided the impetus for the government to consider possible remedies. In addition to a substantially larger number of public enforcement actions taken by the regulatory agencies during 2001, the Supreme Court issued a Notice on January 15, 2002 that it would accept civil lawsuits against false statements in the securities market. Investors responded with a wave of lawsuits. In January 2003, the Supreme Court further promulgated the Private Securities Litigation Rules to expand the scope of securities litigation. The Appendix contains a more detailed account of institutional changes in China during our sample period.

Institutional Improvements and Client Dependence

The Supreme Court Notice represents a sea change in private securities litigation in China (*China Securities Journal* 2007a). Consistent with the suggestion that liberalizing the rules governing shareholder litigation is the single most important factor that motivates managers and auditors to improve financial reporting quality (Ball 2001), we expect the changes in the regulatory and legal environment in China to significantly affect auditor behavior. The high competition and low litigation/sanction risk in the pre-2001 period suggest that the market force constraining pressure from large clients was relatively weak, and we thus expect client importance to affect audit quality negatively during this period. However, because of the institutional improvements in 2001, the litigation and sanction risks faced by auditors have risen significantly. As a result, auditors' concern about potential losses from audit failures, particularly those involving large clients, can supersede the economic benefits of retaining problematic large clients. Consequently, for the post-2001 period, we expect the negative impact of client importance on audit quality to diminish.[3]

Level of Analysis

Chinese auditors are required to sign on audit reports in accordance with China's Independent Auditing Standard (CIAS) No. 7, *Audit Report*, issued in 1995. Two CPAs must sign the audit report to clarify who was responsible for the audits performed.[4] In practice, Chinese CPAs are indeed accountable for audit reports in their names: regulatory sanctions for detected audit failures are imposed not only on CPA firms, but also on individual CPA signatories.[5]

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[3] As all Chinese-listed firms have December 31 as their fiscal year-end, the annual audit work for most of the 2001 reports would have been affected by the Notice issued in early 2002. We assume that auditors respond quickly to changes in the environment to maximize their own utility, and thus consider fiscal year 2001 as the first year to be affected by the institutional improvements. In the Appendix, we further justify this choice by showing the immediate market response to the announcement of the Notice.

[4] For partnership CPA firms, the signing auditors should be the engagement CPA plus the partner conducting the final review; for limited liability CPA firms, the signing auditors should be the engagement CPA plus the head of the firm or a deputy head authorized by the head.

[5] For example, in the sanction notice involving HongGuang Company (Stock Code: 600083) and its auditor, ShuDu CPA Firm, it is clearly stated that "as the signing CPAs for the audit reports of HongGuang Company, Wang Yingqin and Zhang Xiuhua are permanently banned from providing any security-related service" (http://www.csrc.gov.cn).

Francis et al. (1999) suggest the use of city-level markets (i.e., offices), rather than firms, as a unit of analysis in audit research. Consistent with this suggestion, Reynolds and Francis (2000) and Craswell et al. (2002) argue that client importance is better analyzed at the local office level because the economic impact of a larger client is more important to any particular local office than to the firm as a whole. Other studies following this approach include Chung and Kallapur (2003), Krishnan (2005), and Gaver and Paterson (2007). DeFond and Francis (2005) further suggest lowering the level of analysis to individual auditors to improve the power of the tests. However, information about individual engagement partners is not generally available except in a very small number of jurisdictions, of which China is one. Although some researchers have investigated the relation between auditor tenure at the partner level and audit quality (Carey and Simnett 2006; Chen et al. 2008), we are the first to examine how client importance affects audit quality at the individual auditor level and compare it with that at the office level over two periods of improving institutional environments for investor protection.

III. RESEARCH METHOD AND EMPIRICAL RESULTS

Data Source and Sample Selection

Our sample period is from 1995 to 2004, with the six years from 1995 to 2000 for the pre-2001 period and the four years from 2001 to 2004 for the post-2001 period. Modified audit opinions (MAOs) were rare in China before 1995. In December 1995, the Chinese Institute of Certified Public Accountants (CICPA) issued the first batch of CIASs, which specify the responsibility of auditors and the content and format of audit reports. As a result, MAOs increased substantially in that year (DeFond et al. 1999). Our data consist of three types: (1) financial statement and stock market information, which we obtained from the China Stock Market and Accounting Research Data Base (CSMAR); (2) audit opinions and the identities of audit firms, audit offices, and signing auditors, which we manually collected from annual reports, supplemented by the auditor database complied by the CSRC (http://assdata.csrc.gov.cn/); and (3) regulatory sanctions against auditors, which we obtained from the CSRC website.

We begin to construct our sample with all the available firm-year observations from the A-share Financial Statement Database of CSMAR. We obtain 9,247 observations for the period 1995 to 2004, from which we delete 36 observations with missing information about auditor identity and 294 observations with insufficient weekly return data to estimate the market model.[6] Our final sample contains 8,917 firm-year observations from 1,368 unique firms.

Measurement of Audit Quality and Client Importance

We use an auditor's propensity to issue a modified audit opinion (MAO) as a proxy for audit quality. Craswell et al. (2002, 255) state that "the presumption underlying this line of inquiry is that, if fee dependence affects auditors' independent judgment, then auditors are less likely to issue qualified audit opinions." This assumption is also applicable in the Chinese context, as Chen et al. (2000) document a negative market reaction to MAO recipients in China. According to the CSRC's disclosure requirements, a firm's management should explain the underlying reasons for an MAO in the annual report. Furthermore, the stock exchanges also consider the nature of audit opinions in their delisting decisions.[7] Therefore, MAOs can be costly to clients, and auditors may compromise by issuing a clean opinion when an MAO is warranted.

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[6] We require at least 15 weekly observations during a year for the market model estimation.

The CIAS specifies four types of audit opinions: unqualified, qualified, disclaimer, and adverse. It also stipulates that explanatory notes can be used with unqualified opinions where necessary. Although the CICPA interprets unqualified opinions with explanatory notes in a manner similar to the "emphasis of a matter" in the U.S., this type of audit report is often issued in place of a qualified opinion (Chen et al. 2000; Haw et al. 2003). The CSRC and the stock exchanges in China also treat this type of report in the same way as other non-clean opinions in disclosure requirements or delisting decisions. To measure the different types of MAOs according to their levels of severity, we code the audit opinion variable *OP* from 0 to 3 to represent clean opinions, unqualified opinions with explanatory notes, qualified opinions, and disclaimers/adverse opinions, respectively. We combine the disclaimers and adverse opinions into one group because there are only four cases of adverse opinions in our sample. In the multivariate analysis, we use an ordered logistic model rather than a dichotomous model, because the former more accurately measures the severity of auditor opinions.[8] The ordered specification also has a higher pseudo R^2 than the dichotomous model, but our main conclusions remain the same even if audit opinions are measured dichotomously.[9]

Table 1 shows the distributions of our final sample and the audit opinions for each year. MAOs account for 13.7 percent of the entire sample, and their relative frequency peaks in 1999 and declines thereafter. This decline in the post-2001 period is mainly due to the following reasons. First, the CSRC issued Listed Company Disclosure Rule No. 14, *Problems under Non-Standard Audit Opinions*, in 2001 (CSRC 2001b). This rule stipulates more stringent requirements for audit adjustments when violations of the GAAP or the regulations are detected by auditors. Second, the CICPA revised the *Audit Reports* standard in 2003 and restricted the use of unqualified opinions with explanatory notes to going-concern and uncertainty issues that do not affect the overall audit opinion. Under this standard, clients are more likely to accept an audit adjustment if an audit opinion other than unqualified is expected. Consistent with this reasoning, we observe less unqualified opinions with explanatory notes in 2003 and 2004 than in the preceding years. Third, with improved investor protection in the post-2001 period, management is also likely to take less risk with accounting manipulations, reducing instances of MAOs. Although the time-series variation in the frequency of MAOs may affect the regression intercepts, we are primarily interested in the slope effect, or how client importance affects the likelihood of MAOs under a given audit-reporting regime.

As audit fees were not disclosed before 2001, we use the natural logarithm of a client's total assets as a surrogate to measure client economic importance. This is suitable because

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[7] In China, a firm that has incurred losses in the previous three years must report a profit with a clean audit opinion in the current year to avoid being delisted by the stock exchange.

[8] Wu and Zhuang (2006) report a monotonic decrease in clients' current and one-year-ahead financial performance from clean opinions to disclaimers/adverse opinions. This indicates that Chinese auditors use different audit opinions to convey information to the market, and justifies the ordered nature of our dependent variable. Haw et al. (2003) also report earnings announcement evidence that is consistent with the severity of audit opinions. Further, regulators consider the appropriateness of audit report types. The CSRC sanctioned the Beijing JingDu CPAs for its audit of ZhongGuanCun in 2002 because of an inappropriate use of explanatory notes for a more severe accounting problem in the company.

[9] More details will be provided about alternative measures of audit opinion in the sensitivity analysis.

TABLE 1
Sample Distribution by Year and Type of Audit Opinion

Opinion	1995	1996	1997	1998	1999	2000	2001	2002	2003	2004	Total
(1) Clean	252 86.6%	367 88.2%	589 86.1%	649 81.7%	712 79.8%	820 82.2%	969 86.8%	1,012 86.8%	1,130 91.9%	1,193 89.6%	7,693 86.3%
(2) Unqualified but with Explanatory Notes	4 1.4%	28 6.7%	52 7.6%	93 11.7%	109 12.2%	107 10.7%	89 8.0%	94 8.1%	57 4.6%	69 5.2%	702 7.9%
(3) Qualified	35 12.0%	21 5.0%	41 6.0%	39 4.9%	57 6.4%	57 5.7%	40 3.6%	42 3.6%	23 1.9%	46 3.5%	401 4.5%
(4) Disclaimer	0 0.0%	0 0.0%	1 0.1%	12 1.5%	13 1.5%	13 1.3%	18 1.6%	18 1.5%	19 1.5%	23 1.7%	117 1.3%
(5) Adverse	0 0.0%	0 0.0%	1 0.1%	1 0.1%	1 0.1%	1 0.1%	0 0.0%	0 0.0%	0 0.0%	0 0.0%	4 0.0%
Total	291	416	684	794	892	998	1,116	1,166	1,229	1,331	8,917

From all of the firm-year observations available in the A-share Financial Statement Database of CSMAR during the 1995–2004 period, we delete: (1) 36 observations that do not reveal the signing auditor's identity; and (2) 294 observations with insufficient weekly return data to estimate the market model.

audit fees in China are typically based on total client assets, and the logarithm transformation takes the nonlinear relationship between audit fees and client total assets into account.[10] We define the importance of client i to audit office j as:

$$CI^{O} = \frac{\mathrm{Ln}TAST_i}{\sum_{i=1}^{n} \mathrm{Ln}TAST_i}, \tag{1}$$

where $\mathrm{Ln}TAST_i$ is the natural logarithm of the total assets of client i and $\sum_{i=1}^{n} \mathrm{Ln}TAST_i$ is the sum of the total assets (in natural logarithm form) of n clients audited by audit office j in a particular year. To minimize the potential measurement error in CI^{O}, we use the whole universe of listed companies in China (which is somewhat larger than the sample used in this study) to compute the base $\sum_{i=1}^{n} \mathrm{Ln}TAST_i$. Similarly, we measure client importance at the auditor level as:

$$CI^{A} = \frac{\mathrm{Ln}TAST_i}{\sum_{k=1}^{m} \sum_{i=1}^{l} \mathrm{Ln}TAST_i}, \tag{2}$$

where l is the number of clients audited by auditor k, and m is the number of auditors who signed the audit report. The CIAS requires that at least two auditors sign an audit report. In our sample, there is a small fraction of reports (1.13 percent, 101 reports) signed by three auditors. Thus, $m = 2$ in most cases, but $m = 3$ for 101 cases.

As the majority of the audit reports are signed by two auditors in a non-alphabetic order, we present two characteristics of these pairs of auditors in Panel A of Table 2 by the order of their signatures on the audit report. The first is the client base, which is measured as the sum of logged total assets audited by the auditors. The mean value suggests that the first auditor has a larger client base, in 58.6 percent of the cases (untabulated). The second characteristic is the length of an auditor's experience, which is defined as the number of years since the auditor first appears in our database (where 0 is the first year that an auditor signed an audit report). As 1995 is the first year in our database, the practice experience variable is computed for the 1998–2004 period to allow for meaningful variation. We find that in 53.2 percent of the observations (untabulated), the first auditor has a longer practice experience than the second. The main results reported later in this section are based on CI^{A} as defined in Equation (2). By measuring importance by the clientele of all relevant auditors, this metric avoids contamination of the results by auditors' signing order or clientele size. We nonetheless employ alternative client importance measures in the sensitivity tests.

Panel B of Table 2 describes client importance at the office and auditor levels. Both CI^{O} and CI^{A} range between 0 and 1, with higher values representing greater importance.

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[10] We also use the logarithm of sales as an alternative proxy for audit fee, and find our main results to be qualitatively the same. In the sensitivity analysis, we report results that are based on audit fees for the 2001–2004 period. It is worth noting that nonaudit services in China are negligibly small. Han and Zhou (2003) report that of the 1,017 Chinese-listed firms that disclosed the fees that they paid to audit firms in 2001, only 28 paid nonaudit consultation fees. Whether nonaudit or total audit fees are a better measure of client economic dependence is thus not a concern in China.

TABLE 2
Description of Audit Office and Signing Auditor Characteristics

Panel A: Characteristics of Signing Auditor (n = 8,917 for client base and 7,527 for practice experience)

	The First Signing Auditor			The Second Signing Auditor		
Characteristic	**Mean**	**Min.**	**Max.**	**Mean**	**Min.**	**Max.**
Client base	107.902	17.707	422.496	81.264	17.591	422.496
Practice experience	3.835	0.000	9.000	2.810	0.000	9.000

Panel B: Descriptive Statistics for the Client Importance Variables (n = 8,917)

Variable	Mean	Min.	Median	Max.	STD
CI^O	0.097	0.011	0.059	1.000	0.127
CI^A	0.240	0.043	0.169	1.000	0.211

Panel C: Contingence Table by CI^O and CI^A

CI^A CI^O	Bottom Quintile	2	3	4	Top Quintile	Total
Bottom quintile	899	411	260	161	76	1,807
2	505	538	390	270	108	1,811
3	247	408	380	438	306	1,779
4	112	313	468	460	394	1,747
Top quintile	40	114	280	467	872	1,773
Total	1,803	1,784	1,778	1,796	1,756	8,917

In Panel A, the first and second signing auditors are defined by the order in which they signed the audit reports. Client base is the natural logarithm of total assets audited by the respective auditors, and practice experience is the number of years since the auditors first appeared in the database, which starts at 1995. The sample period does not cover 1995 to 1997 for the practice experience variable.

In Panel B, CI^O is client importance at the audit office level, which is computed as $\frac{\mathrm{Ln}TAST_i}{\sum_{i=1}^{n}\mathrm{Ln}TAST_i}$, where $\mathrm{Ln}TAST_i$ is the natural logarithm of the total assets of client i and n is the number of clients audited by the audit office in a particular year. CI^A is client importance at the individual auditor level, which is computed as $\frac{\mathrm{Ln}TAST_i}{\sum_{k=1}^{m}\sum_{i=1}^{i}\mathrm{Ln}TAST_i}$, where i is the number of clients audited by auditor k and m is the number of auditors signing the audit report.

In Panel C, CI^O and CI^A are sorted independently by year.

There are 37 (438) extreme observations with CI^O (CI^A) equal to 1, which means that the audit office or auditor has only one listed client. To evaluate the impact of these observations, we conduct a sensitivity analysis after excluding the observations with $CI^A = 1$ in the next section. A contingency table of CI^O versus CI^A is presented in Panel C, in which the observations are independently grouped into CI^O and CI^A quintiles by year. The frequencies are unevenly distributed, with observations clustering in the bottom and top quintiles. The Pearson correlation coefficient between CI^O and CI^A is 0.466, which indicates that a client can be important to both the audit office and the individual auditor. Although the correlation is below 0.80, the point beyond which multicollinearity becomes a real concern, we conduct several tests to evaluate the possible influence of multicollinearity in subsequent analyses.

Empirical Analyses and Results

Multivariate Ordered Logistic Regression

As CI^O and CI^A are correlated with each other and may covary with other determinants of audit opinions, we conduct a multivariate analysis using the following ordered logistic regression model (from which the firm and time subscripts are omitted for simplicity):

$$\begin{aligned} OP = {} & \alpha_0 + \alpha_1 LagOP + \beta_1 Quick + \beta_2 ARINV + \beta_3 Leverage \\ & + \beta_4 Turnover + \beta_5 ROA + \beta_6 Loss + \beta_7 Age \\ & + \beta_8 LnTAST + \beta_9 RET + \beta_{10} Beta + \beta_{11} STDRET \\ & + \beta_{12} EM + \beta_{13} Local + \gamma_1 Expert^O + \gamma_2 Expert^A + \gamma_3 Size^O \\ & + \gamma_4 Size^A + \delta_1 CI^O + \delta_2 CI^A + \Sigma\eta_t YR_t + \Sigma\theta_j IND_j + \varepsilon. \end{aligned} \quad (3)$$

In this model, the dependent variable *OP* (audit opinion) is coded from 0 to 3 for clean, unqualified with explanatory notes, qualified, and disclaimed/adverse opinions, respectively. Our experimental variables are CI^O and CI^A. To evaluate the impact of the 2001 institutional improvements, we run the model for the pre-2001 and post-2001 periods separately. For the pre-2001 period, if audit offices or auditors are more benevolent to economically important clients, then the coefficients on CI^O and CI^A should be negative. For the post-2001 period, if the institutional improvements are to some extent effective, then the coefficients on these two variables should become insignificant or even positive. In sum, we predict a positive change in the *CI* coefficients from the pre- to post-2001 periods.

We control for several factors that could affect audit opinions. The coefficient on *LagOP* (audit opinion in the previous year) is expected to be positive because audit opinions tend to persist. We also include six financial statement and three stock market variables, following past studies on audit reporting in the U.S. (e.g., Dopuch et al. 1987; Bell and Tabor 1991) and China (DeFond et al. 1999; Chen et al. 2001; Wang et al. 2008). The financial statement variables include *Quick* (quick assets, including cash, short-term investments, and accounts receivable, divided by current liabilities), *ARINV* (accounts receivable and inventory divided by total assets), *Leverage* (total liabilities divided by total assets), *Turnover* (total sales divided by total assets), *ROA* (earnings divided by total assets), and *Loss* (an indicator variable that indicates a loss in a given year). We expect the coefficients on *Quick*, *Turnover*, and *ROA* to be negative, and those on *ARINV*, *Leverage*, and *Loss* to be positive. The stock market variables are *RET* (market-adjusted stock returns), which proxies for news that has been incorporated into stock returns but is not yet recognized in earnings; and *Beta* and *STDRET* (the slope coefficient and standard deviation of the residuals, respectively, from the market model) to represent firms' systematic and unsystematic risk.[11] The coefficients on *RET* and the risk proxies (*Beta* and *STDRET*) are expected to be negative and positive, respectively.

We include *Age* (the number of years a company has been listed) and Ln*TAST* (the natural logarithm of clients' total assets) to control for other aspects of clients' financial health. Because Chinese firms are more susceptible than their overseas counterparts to financial distress once they have exhausted the capital raised from their IPOs, younger firms are less likely to receive MAOs (DeFond et al. 1999; Chen et al. 2001). *Age* is expected to have a positive coefficient. Ln*TAST*, as a measure of client firm size, is expected to have a negative coefficient, as large firms usually have more stable operations and better internal control systems.

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[11] We estimate the market model by regressing individual stock returns on the market return using weekly stock return data from the fiscal year.

Certain patterns of earnings management in China have been reported in the literature (Chen and Yuan 2004; Haw et al. 2005), and some firms receive MAOs due to these activities (Chen et al. 2001). We include an indicator variable, *EM*, to identify those clients that fall into any of the known earnings management patterns.[12] The coefficient on *EM* is expected to be positive. *Local* (which takes the value of 1 for cases where audit offices and client firms are located in the same region) captures the possible political influence of local government. Chan et al. (2006) and Wang et al. (2008) find that auditors report more favorably when the audit office and client firm are located in the same region. The coefficient on *Local* is expected to be negative.

Audit quality is a joint product of auditor competence and independence (DeAngelo 1981). To control for competence, we include industry expertise and auditor size variables. Consistent with previous studies on audit industry specialization (Ferguson and Stokes 2002; Ferguson et al. 2003), we use the indicator variable $Expert^O$ to indicate whether an audit office is the industry leader or has a market share of greater than 10 percent in terms of total assets audited in the industry. Similarly, we include $Expert^A$ to indicate whether any of the signatory auditors is the industry leader or has a market share of greater than 10 percent in the industry.[13] The size variables at the audit office and auditor levels ($Size^O$ and $Size^A$) are the denominators of Equations (1) and (2), respectively. If specialized and large auditors are more competent or more concerned with their reputation (Dopuch and Simunic 1980a, 1980b; DeAngelo 1981; Gul et al. 2003), then the coefficients on the industry expertise and auditor size variables should be positive. Finally, ΣYR_t and ΣIND_j are yearly and industry indicator variables employed to control for variations in the frequency of MAOs over time and across industries.

To mitigate the undue influence of extreme values, we winsorize all of the continuous variables in the bottom and top percentiles of their annual distributions. The descriptive statistics for the control variables are reported in Table 3.

Regression Results

As the same client can appear several times in our final sample and the residuals may be correlated across observations, we use the robust standard errors clustered by client firm (Petersen 2009). The regression results for the pre-2001 and post-2001 periods are reported in Panels A and B, respectively, of Table 4. To save space, the regression intercepts and

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[12] *EM* is coded 1 if an observation meets any of the following criteria: (1) firms with a small profit (0 < ROA ≤ 1 percent), (2) loss-making firms with a reported ROA that is lower than the median value of the non-positive ROAs of all of the listed firms, and (3) firms with ROEs that are marginally above the CSRC's rights offering requirements specified for different periods (10–11 percent before 1999; 6–7 percent or 10–11 percent for 1999–2000; and 6–7 percent for a lower bottom-line ROE or recurring ROE after 2000). Criterion (1) captures a firm's intention to avoid reporting losses, criterion (2) (adapted from Riedl [2004]) involves possible big baths, and criterion (3) indicates earnings management to meet the profitability requirement for rights offerings set by the CSRC (CSRC 1996, 1999, 2001a).

[13] Ferguson and Stokes (2002) find that empirical results regarding the fee premiums earned by specialist auditors are sensitive to industry specialization definitions. The specialization variables are not significant in our sample. Alternative definitions, such as only considering the industry leadership or only using market share (and an alternative market share cutoff of 20 percent) do not alter the results.

TABLE 3
Descriptive Statistics for the Control Variables Used in the Multivariate Ordered Logistic Regressions

Variable	Mean	Min.	Median	Max.	STD
Quick	0.903	0.018	0.632	6.277	0.914
ARINV	0.335	0.011	0.315	1.098	0.182
Leverage	0.492	0.059	0.479	2.436	0.233
Turnover	0.542	0.013	0.440	2.439	0.401
ROA	0.029	−0.514	0.036	0.239	0.077
Loss	0.109	0.000	0.000	1.000	0.312
Age	4.667	0.310	4.269	12.849	2.909
Ln*TAST*	20.891	18.736	20.829	23.885	0.899
RET	−0.006	−0.678	−0.054	2.456	0.302
Beta	1.025	−0.380	1.021	2.684	0.326
STDRET	0.040	0.004	0.037	0.295	0.017
EM	0.336	0.000	0.000	1.000	0.472
Local	0.706	0.000	1.000	1.000	0.456
$Expert^{O}$	0.109	0.000	0.000	1.000	0.312
$Expert^{A}$	0.054	0.000	0.000	1.000	0.226
$Size^{O}$	422.127	19.428	353.786	1,826.400	307.204
$Size^{A}$	141.034	19.152	123.894	474.442	88.184

n = 8,917.

Variable Definitions:

Quick = quick ratio (sum of cash, short-term investments, and accounts receivables divided by current liabilities);
ARINV = accounts receivables and inventory intensiveness (sum of accounts receivables and inventory divided by total assets);
Leverage = leverage ratio (liabilities divided by total assets);
Turnover = turnover ratio (total sales divided by total assets);
ROA = return on assets (earnings divided by total assets);
Loss = 1 if the client firm reported a loss, and 0 otherwise;
Age = number of years a company has been listed;
Ln*TAST* = natural logarithm of the total assets of clients;
RET = cumulative market-adjusted stock returns from January to December;
Beta = slope coefficient from the market model estimated using weekly return data over the year;
STDRET = standard deviation of the residuals from the market model estimated using weekly return data over the year;
EM = 1 if the client firm is suspected to have managed earnings to avoid delisting or to qualify for a rights offering, and 0 otherwise;
Local = 1 if the audit firms and client firms are located in the same region (province or equivalent in China), and 0 otherwise;
$Expert^{O}$ = 1 if the audit office is the industry leader or has a market share of greater than 10 percent in terms of total assets audited in the industry, and 0 otherwise;
$Expert^{A}$ = 1 if any of the signing auditors is the industry leader or has a market share of greater than 10 percent in terms of total assets audited in the industry, and 0 otherwise;
$Size^{O}$ = size of audit office $\left(\sum_{i=1}^{n} \mathrm{LN}TAST_i\right.$, where $\mathrm{Ln}TAST_i$ is the natural logarithm of total assets for client i, and n is the number of clients audited by the audit office$\left.\right)$; and
$Size^{A}$ = size of individual auditor $\left(\sum_{k=1}^{m} \sum_{i=1}^{l} \mathrm{Ln}TAST_i\right.$, where i is the number of clients audited by auditor k, and m is the number of auditors signing the audit reports$\left.\right)$.

All of the continuous variables have been winsorized at the bottom and top yearly percentiles.

TABLE 4
Multivariate Ordered Logistic Regression Analysis of Audit Opinions

Panel A: The Pre-2001 Period (1995–2000)

	Specification (1)		Specification (2)		Specification (3)	
Variable	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**
LagOP	1.7468	14.019***	1.7364	13.781***	1.7272	13.752***
Quick	−0.0269	−0.248	−0.0207	−0.189	−0.0279	−0.255
ARINV	1.5746	4.702***	1.6041	4.759***	1.5918	4.709***
Leverage	1.0056	2.596***	1.0235	2.665***	0.9752	2.523**
Turnover	−0.7427	−3.512***	−0.7383	−3.509***	−0.7413	−3.498***
ROA	−5.6086	−4.142***	−5.5704	−4.100***	−5.6603	−4.156***
Loss	0.4720	1.954*	0.4866	2.013**	0.4741	1.960*
Age	0.0507	1.604	0.0574	1.870*	0.0547	1.737*
Ln*TAST*	0.0265	0.402	0.0262	0.392	0.0304	0.456
RET	−0.4760	−2.784***	−0.4637	−2.709***	−0.4744	−2.758***
Beta	−0.5182	−3.492***	−0.5245	−3.558***	−0.5232	−3.552***
STDRET	15.2017	4.507***	14.8105	4.423***	15.2042	4.502***
EM	0.2368	2.065**	0.2479	2.146**	0.2406	2.083**
Local	−0.0054	−0.045	−0.0084	−0.070	−0.0089	−0.074
$Expert^{O}$	−0.1043	−0.606			−0.1639	−0.899
$Expert^{A}$			0.1467	0.718	0.2301	1.065
$Size^{O}$	−0.0004	−1.333			0.0000	0.000
$Size^{A}$			−0.0025	−3.125***	−0.0026	−2.889***
CI^{O}	−1.6782	−2.516**			−1.1717	−1.481
CI^{A}			−1.2091	−3.386***	−0.9281	−2.309**
Wald χ^2	892.76***		896.25***		896.15***	
Pseudo R^2	24.13%		24.24%		24.32%	
n	4,075		4,075		4,075	

Panel B: The Post-2001 Period (2001–2004)

	Specification (1)		Specification (2)		Specification (3)	
Variable	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**
LagOP	2.2183	15.405***	2.2111	15.228***	2.2022	15.261***
Quick	−0.0689	−0.574	−0.0630	−0.537	−0.0786	−0.673
ARINV	1.3606	3.483***	1.4004	3.543***	1.4183	3.592***
Leverage	1.1805	4.994***	1.2061	5.055***	1.1991	5.068***
Turnover	−0.4438	−2.342**	−0.4238	−2.251**	−0.4598	−2.409**
ROA	−1.5036	−2.503**	−1.5033	−2.492**	−1.5362	−2.535**
Loss	1.7063	10.079***	1.6766	9.927***	1.6829	9.911***
Age	−0.0299	−1.317	−0.0260	−1.150	−0.0309	−1.361
Ln*TAST*	0.1305	1.828*	0.1141	1.576	0.1189	1.645
RET	−0.8714	−3.129***	−0.8443	−3.036***	−0.8483	−3.034***
Beta	0.4385	2.546**	0.4301	2.526**	0.4637	2.694***
STDRET	15.5630	3.225***	14.7939	3.097***	15.3979	3.216***

(continued on next page)

TABLE 4 (continued)

	Specification (1)		Specification (2)		Specification (3)	
Variable	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**
EM	0.4333	3.248***	0.4266	3.188***	0.4228	3.158***
Local	−0.2541	−2.030**	−0.2174	−1.765*	−0.2264	−1.811*
$Expert^O$	0.0277	0.119			0.0408	0.169
$Expert^A$			−0.0570	−0.189	−0.0865	−0.281
$Size^O$	0.0002	1.000			0.0003	1.500
$Size^A$			0.0003	0.333	−0.0004	−0.400
CI^O	−0.1796	−0.197			−0.8896	−0.947
CI^A			0.8294	2.422**	0.9259	2.657***
Wald χ^2	1,046.67***		1,047.41***		1,047.61***	
Pseudo R^2	27.88%		27.98%		28.08%	
n	4,842		4,842		4,842	

Panel C: Difference in the Coefficients between the Pre- and Post-2001 Periods

	Specification (1)		Specification (2)		Specification (3)	
Variable	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**
CI^O	1.4986	1.327			0.2821	0.230
CI^A			2.0385	4.120***	1.8540	3.485***

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*, **, *** Denote two-tailed statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

The results are based on the ordered logistic model. The sample period is from 1995 to 2000 in Panel A, and from 2001 to 2004 in Panel B. The dependent variable is audit opinion, and is coded from 0 to 3 for clean, unqualified but with explanatory notes, qualified, and disclaimed/adverse opinions, respectively. The regression intercepts and industry indicator and year indicator variables are not reported. The Z-statistics are based on robust standard errors clustered by firm.

Panel C presents the differences in the coefficients of CI^O and CI^A between Panels A and B. The Z-statistics for the coefficient differences are computed as: $Z = (\hat{\beta}_{Pre} - \hat{\beta}_{Post})/\sqrt{s^2(\hat{\beta}_{Pre}) + s^2(\hat{\beta}_{Post})}$, where $\hat{\beta}_{Pre}$ and $\hat{\beta}_{Post}$ are the coefficient estimates from the pre- and post-2001 periods, respectively, and $s^2(\ )$ are the squared standard errors of the coefficients.

The independent variables are as defined in Table 2 and Table 3.

yearly and industry indicator variables are not reported. Panel A shows that when we focus on the office-level (Specification (1)) and auditor-level (Specification (2)) variables separately, CI^O, CI^A , and $Size^A$ are all significantly negative, but $Size^O$ and the two industry expertise variables do not appear to have any impact. However, when the variables for both levels are included in the full model (Specification (3)), CI^A and $Size^A$ are still significant, but CI^O is no longer so. As estimated coefficients in multiple regressions represent the incremental effect of each independent variable on the dependent variable (Theil 1971), the Specification (3) results suggest that CI^O does not have statistically significant explanatory power incremental to that of CI^A. The significantly negative coefficient on CI^O in Specification (1) could reflect the effect of CI^A, which is positively correlated with CI^O but omitted in that regression. In sum, during the pre-2001 period, individual auditors were less likely to issue MAOs for economically important clients. This result also holds for individual auditors with large clienteles.[14] In the sensitivity analysis, we further evaluate whether the insignificant result for the CI^O is caused by its correlation with CI^A.

We conduct similar tests for the post-2001 period, as reported in Table 4, Panel B. The office-level variables are not significant in Specification (1), but CI^A is significantly positive in Specification (2). None of the *Expert* or *Size* variables are significant. The results from the full model in Specification (3) confirm those from the first two regressions. Therefore, after the changes of the institutional environment in 2001, individual auditors became more conservative and were more likely to issue MAOs to larger clients. This is consistent with our conjecture that institutional improvements affect auditors to such an extent that concerns about litigation, sanctions, and career protection take precedence over the economic dependence on large clients.

To test whether the main coefficients are the same across the two periods, we use the following Z-statistics:

$$Z = \frac{(\hat{\beta}_{\text{Pre}} - \hat{\beta}_{\text{Post}})}{\sqrt{s^2(\hat{\beta}_{\text{Pre}}) + s^2(\hat{\beta}_{\text{Post}})}}, \tag{4}$$

where $\hat{\beta}_{\text{Pre}}$ and $\hat{\beta}_{\text{Post}}$ are coefficient estimates from the two sample periods and $s^2(\,)$ are the squared standard errors of the coefficients. Z-statistics are appropriate for assessing the significance of differences in regression coefficients between large samples drawn independently (Clogg et al. 1995).[15] The results in Table 4, Panel C show that for all the three specifications, the coefficients on CI^A are significantly different between the two periods, whereas those on CI^O are not. Therefore, the institutional change has a more significant impact at the individual auditor level, which makes a greater contribution to explaining the variation in the incidence of MAOs.

Overall, our results provide evidence of impaired audit quality with important clients at the individual auditor level for the period with weaker institutions for investor protection. However, as expected, later institutional improvements pushed auditors to become more conservative with larger clients in the post-2001 period. The office-level measure is either insignificant or sensitive to the model specification. With the caveat of the correlation between the partner- and office-level measures of client importance, our evidence supports the use of individual auditor-level variables to gain a better understanding of auditor reporting decisions, as suggested by DeFond and Francis (2005).

We also apply the Fama and MacBeth (1973) approach by running the by-year logistic regressions and making statistical inferences according to the distribution of yearly regression coefficients (Fama and French 2001). For simplicity, we only report the main coefficients of interest, CI^O and CI^A, in Table 5. For the pre-2001 period (Panel A), the mean of the coefficients on CI^O is not significantly different from zero in the t-test. Moreover, coefficients are negative in four out of the six years, and are not significant in the binomial test. The coefficients on CI^A are all negative during the six years and significant at the 5 percent level in the binomial test. The t-test also rejects the null hypothesis that the mean of the CI^A coefficients is zero. For the post-2001 period (Table 5, Panel B), CI^O is positive

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[14] This is consistent with the "flight from audit quality" phenomenon that occurred in China's audit market during the 1990s—auditors reluctant to go along with clients were likely to lose those clients due to the lack of demand for quality audit services (DeFond et al. 1999).

[15] Although the differences in coefficients between the two samples can also be compared by pooling the samples and adding an indicator variable and an interaction term to the model, this requires the assumption that the error variance is the same between the samples. We choose to report the Z-statistics, but the conclusion remains the same with the pooling approach.

TABLE 5
The Fama-MacBeth Approach to the Multivariate Ordered Logistic Regression Analysis of Audit Opinions

Panel A: The Pre-2001 Period (1995–2000)

	CI^O	CI^A
Mean coefficient	−2.3848	−1.2232
t-statistic	−1.419	−5.242***
# positive: # negative	2:4	0:6
p-value for the binomial test	0.234	0.016

Panel B: The Post-2001 Period (2001–2004)

	CI^O	CI^A
Mean coefficient	−0.2895	0.8624
t-statistic	−0.366	6.218***
# positive: # negative	2:2	4:0
p-value for the binomial test	0.375	0.063

Panel C: Difference between the Pre- and Post-2001 Periods

	CI^O	CI^A
Difference in the mean coefficients	2.0954	2.0855
t-statistic for the difference in mean	0.956	6.693***

*** Denotes two-tailed statistical significance at the 1 percent level.

The results are based on the Fama and MacBeth (1973) approach. The models are the same as those in Table 4, but only CI^O and CI^A are reported.

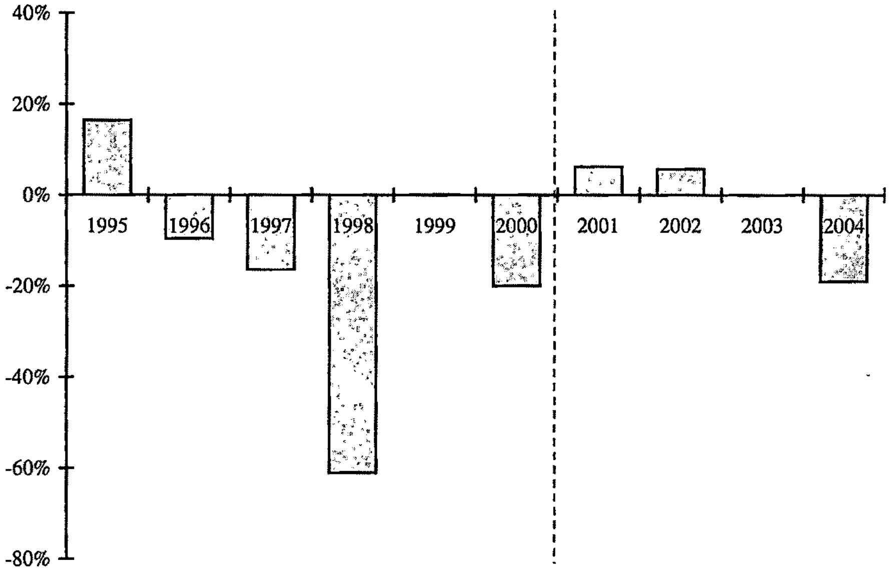
The coefficients are the means of the coefficients from the yearly logistic regressions, and the t-statistics are based on the distribution of these yearly regression coefficients. # positive (# negative) denote the number of times that the regression coefficients are positive (negative).

in two out of the four years, and again is not significant in the t- or binomial tests. The coefficients on CI^A are positive in all four years ($p < 10$ percent in the binomial test) and the mean is also significantly positive in the t-test. Finally, we use the t-test to examine the differences in coefficients between the two periods in Table 5, Panel C. Results indicate that the mean CI^A coefficients in the second period are significantly higher than those in the first period, but that there is no significant difference in the mean CI^O coefficients between the two periods. Overall, the annual regression results corroborate our findings from the pooled regressions.

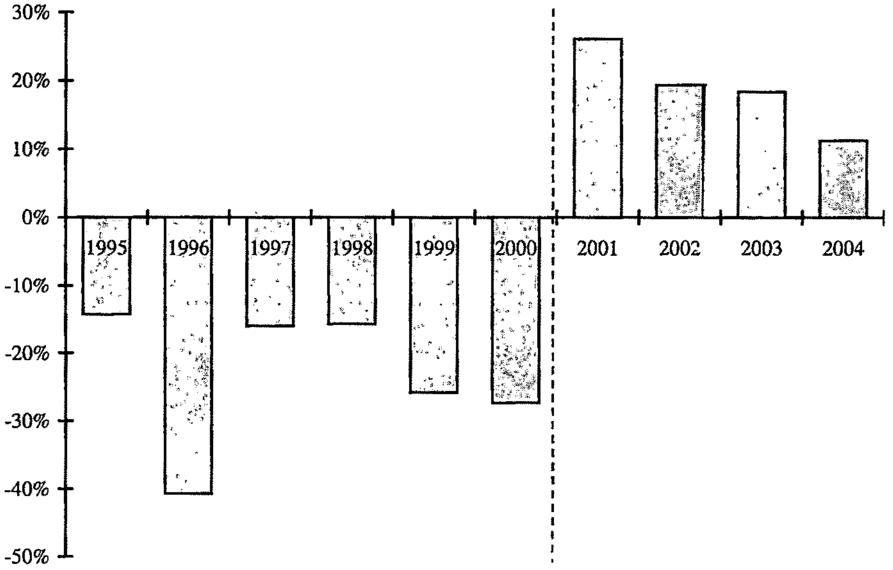
To evaluate the economic significance of the results, we estimate the marginal effect of the two *CI* variables. Based on the full specification of Equation (3), we first estimate the base-rate of MAOs by setting all of the variables to the mean values. The marginal effect is defined as the relative percentage change in the probability of a modified opinion in response to a one-standard-deviation increase in the variables of interest. Figure 1 presents the marginal effects for each sample year. There is no discernable pattern to the CI^O variable (Panel A) over time, and its negative effect on MAOs during the pre-2001 period (see Table 4) appears to be driven by 1998. In Panel B of Figure 1, we observe that a one-standard-deviation increase in the CI^A variable *decreases* the probability of MAOs in all years before 2001, with a mean effect of about 23 percent. This effect, however, becomes

FIGURE 1
The Time-Series of the Marginal Effects of CI^O and CI^A on Auditor Opinions

Panel A: Based on CI^O



Panel B: Based on CI^A



The marginal effects are determined as follows:

1. All of the variables in Equation (3) are set to the annual sample mean values and the base-rate probability of MAO issuance is estimated.
2. The variables of interest (CI^O in Panel A or CI^A in Panel B) are increased by one standard deviation in the respective annual samples and the probability of MAO issuance is re-estimated.
3. The re-estimated probability is divided by the base-rate probability.

predominantly positive after 2001. On average, a one-standard-deviation change in the CI^A *increases* the likelihood of MAOs by about 19 percent. It is worth noting that the change in the effects of CI^A on MAOs does not appear to be caused by any time-series trend during the sample period. Instead, the change unambiguously starts in 2001. This pattern supports the argument that the institutional improvements in that year fundamentally changed the behavior of auditors when auditing large clients.

Firm-Level Analysis

Extant studies of client importance examine audit firm and office level data, so we also analyze the influence of client importance at the firm level. The firm-level variable CI^F is defined as the natural logarithm of the total assets of a particular client divided by the total assets (also in logarithm form) audited by the audit firm in the year. In the early years, most Chinese audit firms had only one office, and CI^O is basically equivalent to CI^F. For our data, the correlations between CI^O and CI^F are all above 0.9 between 1995 and 1999. Starting from 2000, when audit firms began to expand and more local offices were established, the correlation begins to decrease and drops to about 0.6 in the following years. Given the similarity between the firm and office level data, we focus on the impact of CI^F versus CI^A.

Table 6 reports the regression results for the firm-level measure. The regression model is specified as Equation (3), except that we replace the office-level *Expert*, *Size*, and *CI* variables with their corresponding firm-level measures. To be parsimonious, we only present the results for the CI^F and CI^A variables. Columns (1) and (2) show the results obtained from the pooled regressions and Fama-MacBeth annual regressions, respectively. Overall, CI^A performs as well as in the previous analyses: with both the pooled and Fama-MacBeth approaches it is significantly negative in the pre-2001 period and significantly positive in the post-2001 period. The difference between the two periods is also significant. The pattern for the CI^F variable is similar to that of CI^O, except that it is significantly negative in the pre-2001 period in the pooled regression. However, this result is not stable over the years: the coefficient is positive in two out of the six years and insignificant at any conventional level in both the Fama-MacBeth type t-test and binomial test. An untabulated analysis reveals that the negative coefficient in the pooled regression is mainly driven by 1998: the coefficient on CI^F is -10.81 in that year, compared with a mean of -0.99 in the other years. CI^F also becomes insignificantly different from zero ($t = -1.346$) once 1998 is excluded from the pooled sample. Taken together, the evidence in Table 6 suggests that the measurement at the individual auditor level consistently explains auditor behavior in the presence of the firm-level measure.

IV. SENSITIVITY ANALYSES

Measurement of Client Importance

We first analyze whether the results are robust to different *CI* measures. In the foregoing tests, CI^A was based on the total assets audited by all of the relevant auditors. In Table 7, we further measure CI^A based on the auditor with the largest total audited assets (Panel A) and the auditor with the longest experience (Panel B).[16] These alternative measures assume that clientele size or seniority helps to identify the auditor who plays a more important role

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[16] The sample period in Panel B is from 1998 to 2004. The 1995 to 1997 data are not included to allow for variation in the practice experience variable, which we are able to measure from 1995 onward.

TABLE 6
Multivariate Ordered Logistic Regression Analysis of Audit Opinions at the Audit Firm Level

Panel A: The Pre-2001 Period (1995–2000)

	(1) Pooled Regression Approach			(2) Fama-MacBeth Approach	
	CI^F	CI^A		CI^F	CI^A
Coefficient	−1.8886	−0.7516	Mean coefficient	−2.6271	−0.8849
Z-statistic	−2.678***	−1.986**	t-statistic	−1.421	−2.550*
			# positive: # negative	2:4	1:5
			p-value for the binomial test	0.234	0.094*

Panel B: The Post-2001 Period (2001–2004)

	(1) Pooled Regression Approach			(2) Fama-MacBeth Approach	
	CI^F	CI^A		CI^F	CI^A
Coefficient	−1.1297	1.0035	Mean Coefficient	−2.3102	1.0211
Z-statistic	−1.042	2.753***	t-statistic	−1.010	5.516**
			# positive: # negative	1:3	4:0
			p-value for the binomial test	0.250	0.063*

Panel C: Difference between the Pre- and Post-2001 Periods

	(1) Pooled Regression Approach			(2) Fama-MacBeth Approach	
	CI^F	CI^A		CI^F	CI^A
Difference in the coefficient	0.7589	1.7551	Difference in mean coefficient	0.3169	1.9060
Z-statistic	0.587	3.340***	t-statistic	0.108	4.164***

*, **, *** Denote two-tailed statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

The regression model is the same as Specification (3) in Table 4, except that $Expert^O$, $Size^O$, and CI^O are replaced by $Expert^F$, $Size^F$, and CI^F (the respective variables at the audit firm level). Only the CI^F and CI^A variables are reported.

For the pooled regressions, the Z-statistics are based on robust standard errors clustered by firm. For the Fama-MacBeth approach, the coefficients are the means of the coefficients from the yearly logistic regressions, the t-statistics are based on the distribution of these yearly regression coefficients, and # positive (# negative) denote the number of times that the regression coefficients are positive (negative).

in an audit. In general, the results are consistent with those already reported. CI^A is significantly negative and positive in the pre- and post-2001 periods, and the difference between the two periods is also statistically significant. CI^O is significantly negative in Panels B but not in Panel A during the pre-2001 period. Consistent with the main results, CI^O is never significant in the post-2001 period and there is no significant change in this variable between the two periods.[17]

We also conduct an analysis after deleting 438 observations with CI^A equal to 1, which means that the auditor had only one listed client. In such cases, client importance can be

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[17] Measuring CI^A based on the first auditor who signed the report provides the same results qualitatively.

TABLE 7
Sensitivity Analysis of Alternative Measurements (Client Importance and Audit Opinion)

Panel A: Using Auditors with the Largest Client Base to Measure CI^A

	Pre-2001 Period (1995–2000)		Post-2001 Period (2001–2004)		Difference	
Variable	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**
CI^O	−1.2600	−1.620	−0.8638	−0.918	0.3962	0.325
CI^A	−0.8541	−2.100**	0.9957	2.788***	1.8498	3.418***

Panel B: Using Auditors with the Longest Experience to Measure CI^A

	Pre-2001 Period (1995–2000)		Post-2001 Period (2001–2004)		Difference	
Variable	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**
CI^O	−2.5521	−2.467**	−0.5182	−0.554	2.0339	1.458
CI^A	−0.6121	−1.853*	0.5466	2.040**	1.1587	2.724***

Panel C: Excluding Observations with $CI^A = 1$

	Pre-2001 Period (1995–2000)		Post-2001 Period (2001–2004)		Difference	
Variable	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**
CI^O	−0.5126	−0.458	−1.1386	−0.933	−0.6260	−0.378
CI^A	−1.8246	−2.432**	0.5460	0.634	2.3706	2.076**

Panel D: Using Audit Fees to Measure CI^O and CI^A

	Pre-2001 Period (1995–2000)		Post-2001 Period (2001–2004)		Difference	
Variable	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**
CI^O	NA	NA	−0.4172	−0.896	NA	NA
CI^A	NA	NA	1.0527	3.681***	NA	NA

Panel E: Measuring MAOs Dichotomously (1 for all MAOs, and 0 for clean reports)

	Pre-2001 Period (1995–2000)		Post-2001 Period (2001–2004)		Difference	
Variable	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**
CI^O	−0.5113	−0.629	−0.7359	−0.766	−0.2246	−0.178
CI^A	−1.1667	−2.545**	1.0190	2.556**	2.1857	3.598***

Panel F: Excluding the Disclaimer/Adverse Category

	Pre-2001 Period (1995–2000)		Post-2001 Period (2001–2004)		Difference	
Variable	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**
CI^O	−1.2015	−1.456	−0.2971	−0.317	0.9044	0.724
CI^A	−0.8458	−2.111**	0.8925	2.316**	1.7383	3.127***

(*continued on next page*)

TABLE 7 (continued)

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*, **, *** Denote two-tailed statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

The regression model is the same as Specification (3) in Table 4, but only the CI^O and CI^A variables are reported. The Z-statistics are based on robust standard errors clustered by firm.

In Panel A, we measure CI^A as client importance to the auditor with the largest total assets audited.

In Panel B, we measure CI^A as client importance to the auditor with the longest practice experience. The sample period does not cover 1995 to 1997.

In Panel C, we exclude observations in which the auditor has only one client.

In Panel D, the *CI* variables are measured by audit fees. Only the results for the 2001–2004 period are presented, as audit fee data are not available before 2001.

In Panel E, we measure MAOs as a dichotomous variable (1 for all MAOs, and 0 for clean reports).

In Panel F, we exclude all the disclaimer and adverse cases from the sample.

exaggerated if the majority of the auditor's clients are non-listed companies. Results in Table 7, Panel C show that, although the coefficient on CI^A is not significantly different from zero after 2001, the difference between the two periods remains significant at the 5 percent level. This finding indicates that potential measurement error is unlikely to drive the significant change in the effect of the CI^A variable on the propensity for MAOs from the pre- to the post-2001 period. Finally, we use audit fees available after 2001, rather than Ln*TAST*, to measure the *CI* variables. Results in Table 7, Panel D from 4,796 observations for the post-2001 period remain qualitatively the same.

Measurement of Audit Opinion

Next, we examine whether a dichotomous measurement of MAOs (1 for MAOs and 0 for clean opinions) affects the robustness of our results. As shown in Table 7, Panel E, although none of the CI^O variables is significant, the effects of client importance measured at the individual auditor level (CI^A) are still present. Further, to eliminate the possibility of the results being driven by extreme audit opinions, we rerun the tests after dropping all of the disclaimer and adverse cases. Again, the evidence in Panel F is consistent with our main findings.[18]

Additional Sensitivity Analyses

Finally, we address several econometric issues. The continuous metric of the *CI* variables assumes a linear relation between client importance and audit reporting. To relax this assumption, we use an annual quintile rank variable (from 0 to 4) to measure *CI*. We scale the rank variable by four so that the rank ranges between 0 and 1. The inference based on the rank variables (Panel A of Table 8) is similar to the results already reported, except that the coefficient on $R(CI^A)$ becomes insignificant in the post-2001 period. However, as predicted, the difference between the two periods remains significant at the 5 percent level.

Although there is no statistical evidence of severe multicollinearity problems, we make an additional effort to address this concern.[19] As multicollinearity is inherently a data problem rather than a statistical one (Christie et al. 1984), we attempt to address it by identifying

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[18] In our sample, 28 MAOs were purely due to consistency exceptions (i.e., changes in accounting policies). Excluding these cases does not alter our conclusions.

[19] As has been mentioned, the correlation between CI^O and CI^A is below the conventional critical level. In addition, the VIFs estimated from the OLS regression for CI^O and CI^A are 1.59 and 2.12, respectively, for the pre-2001 period, and 1.64 and 2.25, respectively, for the post-2001 period. All of these VIF estimates are far less than 10, the value that is perceived to trigger a serious multicollinearity problem.

TABLE 8
Additional Sensitivity Analyses

Panel A: Using the Quintile Rank Variables to Measure *CI*

	Pre-2001 Period (1995–2000)		Post-2001 Period (2001–2004)		Difference	
Variable	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**
$R(CI^O)$	−0.4519	−1.366	−0.3329	−1.044	0.1190	0.259
$R(CI^A)$	−0.6993	−2.156**	0.4963	1.240	1.1956	2.321**

Panel B: Excluding Observations with a High $\rho(CI^O, CI^A)$

	Pre-2001 Period (1995–2000)		Post-2001 Period (2001–2004)		Difference	
Variable	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**
CI^O	−0.5795	−0.526	−1.2597	−1.013	−0.6802	−0.409
CI^A	−0.7012	−1.688*	0.8193	2.074**	1.5205	2.653***

Panel C: Deleting the Outliers Identified by Deviance Residuals

	Pre-2001 Period (1995–2000)		Post-2001 Period (2001–2004)		Difference	
Variable	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**
CI^O	−2.5355	−1.636	−0.8491	−0.703	1.6864	0.859
CI^A	−1.6779	−2.485**	1.1093	2.171**	2.7872	3.292***

Panel D: Excluding Client Firms Listed after 2000

	Pre-2001 Period (1995–2000)		Post-2001 Period (2001–2004)		Difference	
Variable	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**
CI^O	−1.1717	−1.481	−1.0399	−1.057	0.1318	0.104
CI^A	−0.9281	−2.309**	0.8705	2.367**	1.7986	3.301***

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*, **, *** Denote two-tailed statistical significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

The regression model is the same as Specification (3) in Table 4, but only the CI^O and CI^A variables are reported. The Z-statistics are based on robust standard errors clustered by firm.

In Panel A, we use the scaled quintile rank variables to measure CI^O and CI^A. The quintile ranks are scaled by 4 so that the rank variables range from 0 to 1.

In Panel B, we exclude observations with the difference in percentile rank between CI^O and CI^A below the 5 percent threshold.

In Panel C, we delete the outliers. An observation is treated as an outlier if its deviance residual is significant in the Chi-square statistics at the 5 percent level.

In Panel D, we delete client firms listed after 2000 so that the sample composition is basically the same for the two periods.

observations that cause a high correlation. To do so, we compute the difference between CI^O and CI^A in annual percentile rank. A lower absolute difference would mean that the importance of a client to the audit office is similar to its importance to the individual auditor. For such observations, it may be difficult to disentangle the effects of CI^O versus CI^A. We thus exclude 1,679 observations for which the absolute difference is below 5 percent (i.e., a rule-of-thumb threshold for materiality). The Pearson correlation coefficient between CI^O and CI^A is 0.857 for these excluded observations. The correlation is reduced from 0.466 in the full sample to 0.264 in the trimmed sample. The logistic regression results for the reduced sample in Panel B of Table 8 are similar to those reported earlier. As the multicollinearity concern arises from the significantly negative CI^O coefficient in the model without control for CI^A during the pre-2001 period (i.e., Specification (1) in Panel A of Table 4), we re-estimate the same specification for this trimmed sample and find that the coefficient on CI^O is *not* significant before 2001 ($Z = -0.516$) or after 2001 ($Z = -0.816$). Therefore, the significantly negative impact of CI^O on MAO issuance before controlling for CI^A during the pre-2001 period is mainly attributable to the observations for which CI^O is highly correlated with CI^A. For such observations, the client importance to the office is almost the same as that to the individual auditors. This result confirms our earlier conjecture that the office-level result in Specification (1) before 2001 could have actually captured the effect of the auditor-level variable when it is omitted from the model.[20]

To deal with the problem of influential observations, we define outliers as those with a deviance residual that is significant at the 5 percent level in a Chi-square test (Pregibon 1981). This procedure identifies 539 outliers. The logistic regression results without the outliers in Table 8, Panel C are qualitatively the same as before.

Our sampling method includes every firm-year with necessary data to maximize the sample size and its representativeness. However, as more companies were listed in later years, the different results between the two periods could be due to the change in sample composition for the post-2001 period. To mitigate this concern, we delete the client firms that were listed after 2000 so that the sample composition is basically the same for the two periods. We repeat the analyses on this truncated sample and find our main results to be unaltered in Panel D of Table 8.

V. CLIENT IMPORTANCE AND REGULATORY SANCTIONS

We further test the consequences of auditor reporting decisions by examining the sanctions taken by the CSRC against auditors. Regulators can sanction auditors if misreporting by listed companies is detected and the auditor fails to issue an appropriate audit opinion. If auditors hesitate to issue MAOs to important clients, then we would expect a positive association between client importance and the probability of being sanctioned, particularly for the pre-2001 period. However, for the post-2001 period with improved institutions, we expect this association to dissipate at both levels of *CI* measurement.

From the announcements made on the CSRC website, we identify 88 client firm-years sanctioned by the regulators for problematic audits. These are the firm-years in which sanctioned audit failure occurred, rather than those in which the sanction of the audit failure was announced. For example, if the CSRC announced a sanction in 2002 against failed audits for the fiscal years of 1999 and 2000, we code both 1999 and 2000 (instead of 2002) as the sanctioned firm-years. Therefore, our test is not about whether audit failures were more likely to be investigated and sanctioned during the post-2001 period; rather, we examine the relationships between the incidence of sanctioned audit failures and client importance over the two periods.[21]

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[20] In the trimmed sample, the CI^A variable coefficient is significantly negative ($Z = -1.762$) and positive ($Z = 2.102$) before and after 2001, respectively, when CI^O is not added to the model.

[21] Nonetheless, the untabulated descriptive statistics of our sanction sample support the view that audit failures were more likely to be investigated in the post-2001 period. Out of the 88 sanction cases, 29 were announced from 1995 to 2000, with an average of 4.83 per year, while 59 were announced between 2001 and 2007, with an average of 8.42 per year.

After merging these firm-years with our main sample, 81 sanctioned firm-years involving 52 unique firms remain. Consistent with Lennox and Pittman (2008), we adopt a matched design. For each sanctioned firm, we find a matched control firm in the same industry with the closest logarithm of total assets at the beginning of the year in which the sanctioned audit failure took place. The matching is successful, as there is no significant difference in the size between the sanctioned and control samples (t = 0.182 and Z = 0.241 for the difference in the mean and median, respectively). Due to the small sample size, we pool the two sample periods to estimate the following model:

$$\begin{aligned} Sanction = {} & \alpha_0 + \beta_1 ARINV + \beta_2 Turnover + \beta_3 Leverage + \beta_4 Capital \\ & + \beta_5 STDRET + \beta_6 EM + \beta_7 Local + \gamma_1 Expert^O + \gamma_2 Expert^A \\ & + \gamma_3 Size^O + \gamma_4 Size^A + \delta_0 POST2001 + \delta_1 CI^O \\ & + \delta_2 CI^O \times POST2001 + \delta_3 CI^A + \delta_4 CI^A \times POST2001 + \varepsilon. \end{aligned} \tag{5}$$

The dependent variable, *Sanction*, is equal to 1 for the sanctioned firm years and 0 for the matched control firm years. Based on previous research into accounting fraud (Feroz et al. 1991; Dechow et al. 1996) or alleged audit failures (Stice 1991), we include several control variables, most of which are the same as before. One new variable is *Capital*, an indicator variable indicating whether a firm raised equity capital from the stock market in years $t-1$, t, or $t+1$. This is used to capture instances of earnings management in firms raising equity capital. *POST2001* is another new indicator variable equal to 1 for observations from the post-2001 period. All of the other independent variables are as defined previously.

We report three specifications in Table 9. As the results are similar, we focus our discussion on Specification (3), which includes both CI^O and CI^A. The significantly positive coefficient on CI^A indicates that the probability of sanctioned audit failures before 2001 is higher for clients that are important to individual auditors. This implies that signatory auditors may have compromised with influential clients by issuing favorable audit opinions. However, the interaction between CI^A and *POST2001* is significantly negative, which suggests that the probability of such audit failures is much lower in the post-2001 period. This implies that during the post-2001 period with stronger institutions for investor protection, individual auditors took greater care in auditing their economically important large clients. As for the office-level measure, neither the main effect CI^O nor its interaction with *POST2001* is significant. Overall, these results corroborate our primary findings regarding the effect of client importance on audit-reporting decisions and the impact of institutional improvements.

VI. CONCLUSION

Motivated by the intensifying worldwide interest in, but insufficient evidence of, whether the economic importance of a client impairs audit quality, we investigate this issue within the context of China by (1) measuring client importance at both the audit office and individual auditor levels and (2) examining the impact of institutional improvements on the relationship between client importance and audit quality. Based on a sample of 8,917 client firm-year observations from 1995 to 2004, we document several findings. First, client importance measured at the individual auditor level, but not at the audit office level, impairs audit quality in China in terms of a lower probability of MAO issuance during our pre-2001 period (1995–2000), when legal and regulatory institutions were weak. Although the

TABLE 9
Multivariate Logistic Regression Analysis of the Association between Regulatory Sanctions and Client Importance

	Specification (1)		Specification (2)		Specification (3)	
Variable	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**	**Coefficient**	**Z-statistic**
Intercept	−1.220	−1.007	−2.070	−1.407	−2.107	−1.391
ARINV	0.504	0.360	0.525	0.380	0.531	0.382
Turnover	−0.935	−1.467	−0.742	−1.285	−0.753	−1.256
Leverage	1.237	1.112	1.431	1.285	1.429	1.276
Capital	0.780	1.670*	0.810	1.751*	0.815	1.753*
STDRET	23.269	2.301**	24.139	2.387**	24.199	2.383**
EM	0.316	0.708	0.327	0.746	0.343	0.772
Local	−0.507	−0.826	−0.694	−1.170	−0.682	−1.138
Expert^{O}	−0.110	−0.136	0.016	0.020	0.012	0.014
Expert^{A}	−0.510	−0.548	−0.730	−0.889	−0.714	−0.863
Size^{O}	−0.001	−0.917	−0.001	−1.091	−0.001	−1.000
Size^{A}	−0.002	−0.576	0.001	0.119	0.000	0.098
POST2001	0.947	1.319	1.616	2.238**	1.582	2.092**
CI^{O}	1.507	0.959			−0.098	−0.065
$\mathit{CI}^{O} \times \mathit{POST2001}$	−3.300	−0.901			1.058	0.269
CI^{A}			2.561	1.654*	2.627	1.701*
$\mathit{CI}^{A} \times \mathit{POST2001}$			−3.826	−2.348**	−4.091	−2.396**
Wald χ^2	21.85*		25.25**		25.20*	
Pseudo R^2	15.48%		18.27%		18.31%	
n	162		162		162	

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*, ** Denote two-tailed statistical significance at the 10 percent and 5 percent, respectively.

The results are based on the logistic model. The dependent variable is equal to 1 for client firm-years in which sanctions were applied by the regulators, and 0 for the matched control client-firm years. The matching is based on industry and firm size at the beginning of the sanction year.

Capital is an indicator variable that indicates whether a firm raised equity capital from the stock market in years $t-1$, t, or $t+1$. *POST2001* is an indicator variable for observations from 2001 to 2004. The Z-statistics are based on robust standard errors clustered by firm. The other independent variables are as defined in Table 2 and Table 3.

high correlation between office- and auditor-level measures of client importance during this period may impede us from being more conclusive, our analysis of the observations that drive such a high correlation suggests that the effect of office-level measure is more likely to be an artifact of the auditor-level effect on audit reporting. Second, such impairment disappears and individual auditors become more conservative with economically important clients in our post-2001 period (2001–2004), which is characterized by an improved legal and regulatory environment. However, we find no such evidence for clients that are important to audit offices or firms. Finally, our main findings are corroborated by an analysis of regulatory sanctions. The probability of audit failures sanctioned by the Chinese regulator is significantly higher for clients that were more important to individual auditors in the pre-2001 period, which implies impaired audit quality with large clients. This probability is, however, significantly lowered for the post-2001 period. Again, we find no such evidence when measuring client importance at the office or firm level.

We attribute these results to the trade-off by individual auditors between economic importance and concerns for litigation and/or sanctions under different investor protection environments in China. Before 2001, when the institutional environment for investor protection was weak, economic dependence appeared to dominate. However, with the subsequent institutional improvements in 2001, auditor concerns about litigation risks and regulatory sanctions took priority. We provide evidence that is not only consistent with traditional auditing theory about client dependence (DeAngelo 1981), but also supportive of the moderating effect of institutions (Ball 2001). Although our findings have implications for any regime concerned about the potentially negative impact of client economic dependence, they are particularly relevant to transitional economies in which institutions are generally weak. To restrain the undue influence of large clients on audit quality, our results suggest that policymakers in transitional economies should first consider improvement of the institutions governing investor protection and shareholder litigation.

As compared to the firm- or office-level measure, we find that client importance measured at the individual auditor level seems to be more powerful in capturing audit-reporting decisions. However, the external validity of this finding should be corroborated in other economies, such as Australia or Taiwan, in which individual partners can be identified. It is important to note that in such economies, individual auditors are explicitly accountable for the audit reports that they sign. In most countries in the world, however, audit reports are issued in the name of the audit firm or office, and the risk of audit failures can be diversified among the partners to some extent. Hence, our findings do not necessarily generalize to other countries. Nevertheless, our results suggest that audit accountability at the individual level makes auditors more sensitive to the costs of audit failure. This finding suggests a possible policy implication for the accounting profession. Namely, if our findings can be replicated in other settings where data are available, then it would seem that policymakers should consider initiatives to promote auditor accountability at the individual partner level.

APPENDIX
INSTITUTIONAL CHANGES IN CHINA AROUND 2001

Scandals

The Chinese stock market was beset by a series of scandals starting in early 2000. ZhengBaiWen (Stock Code: 600898), a department store, had been outperforming its peers since it was listed in 1996 and was ranked top in the wholesale/retail industry based on its accounting performance. This performance, however, was achieved through rampant earnings management. In 1999, ZhengBaiWen could not hide its huge losses any longer and reported a bottom line figure of about a 1 billion RMB net loss. Suffering severe going-concern problems, the company was served a notice of bankruptcy in March 2000. The case was so serious that in October 2000, the Xinhua News Agency, the "mouthpiece" of the Chinese central government, published an article to reprimand the company. Four months later, in February 2001, came the scandal of MonkeyKing (Stock Code: 000535), the controlling shareholder of which went bankrupt after draining the resources of its listed subsidiaries. The collapse of the parent company resulted in MonkeyKing's assets being insufficient to meet its obligations. Then, in April 2001, the CSRC released the results of its investigation into Yorkpoint (Stock Code: 000008). By manipulating Yorkpoint's stock price and making false statements between 1998 and 2000, the firm's top managers, together with four fund management firms, reaped profits of 0.45 billion in RMB. In August 2001, *CaiJing* (2001) exposed an even more disturbing story about the Chinese "Enron," YinGuangXia (Stock Code: 000557), which was at the time deemed the No. 1 blue chip stock in China. By fabricating sales and non-existent plants, this company manipulated its earnings to support a fourfold increase in its share price in 2000. Its auditor, ZhongTianQin, then the largest audit firm in China, had issued clean opinions on the company for five consecutive years from 1996 to 2000. This was deemed to be a very serious audit failure by the CSRC, which led to the revocation of the ZhongTianQin's license in early 2002 and the eventual demise of the firm.

Improvements

All of these scandals contributed to a market crash. The Shanghai Stock Exchange Composite Index tumbled from a high of 2225 in June 2001 to 1774 by the end of September. Furious investors started a wave of lawsuits demanding compensation. A total of 391 investors filed suits against Yorkpoint, YinGuangXia, and other parties responsible in big cities such as Beijing and Guangzhou (Chen 2003). Surprisingly, the Chinese Supreme Court, instead of making efforts to calm investors down, issued a notice in September directing "all lower courts temporarily *not* to accept private securities lawsuits." This decision not only pushed investor anger to an even higher level, but also put the Court under the spotlight (Chen 2003).

Coffee (2001) argues that crashes produce victims and hence a political demand for legislative reform. Consistent with this "crash-then-law" model, these events provided the impetus for the government to work on possible remedies. The number of public enforcement actions taken by regulatory agencies, including the CSRC and the Shanghai and Shenzhen stock exchanges, reached its peak of 71 cases in 2001, as compared to only 16 in 2000 and 12 cases in 1999 (Pistor and Xu 2005). In early 2002, the CICPA announced its review decision regarding the qualifications to audit listed companies for a batch of seven CPA firms. These firms, including ZhongTianQin, had their qualifications either revoked or suspended because of poor audit quality, internal quality control or other problems. As such revocation or suspension decisions are normally made on a case-by-case basis, this announcement signifies the strengthened regulatory efforts after the series of financial scandals around 2001 (Li and Wu 2005).

On January 15, 2002, the Supreme Court issued a Notice on accepting civil lawsuits against false statements in the securities market to comfort outraged investors. This Notice dictated that the intermediate courts could accept private securities litigation based on false disclosure or material misinterpretation, provided that the CSRC had investigated and sanctioned the alleged fraud. Investors responded without delay. On the same day, suits against YinGuangXia and its auditor ZhongTianQin were accepted by the Yinchuan and Shenzhen intermediate courts (*Shanghai Securities News* 2002). The next day, lawyers distributed a list of companies that could be sued for false statements and urged investors to take this step (*Securities Times* 2002). Two days later, several investors filed lawsuits against DaQing LianYi (Stock Code: 600065) and its auditor (the Ha'erBin CPA firm) for making false statements in its IPO prospectus. Within a year, 893 civil cases against listed companies and their intermediaries had been accepted by the Chinese courts (*China Economic Times* 2002). The Supreme Court further promulgated the *Private Securities Litigation Rules* in January 2003 and extended the conditions for the acceptance of securities litigation from requiring CSRC sanction to other forms of administrative action or a criminal court ruling. Moreover, the rules made clear that the burden of proof was on the auditors in private securities litigation. By July 2007, a total of seven audit firms, including KPMG and Deloitte, were involved in such litigation (*China Securities Journal* 2007b).

Stock Market Response

Anecdotal evidence on changes in the legal and regulatory environments is consistent with the treatment of the fiscal year of 2001 as a dividing point in our analysis. We further analyze the market response to the Notice to examine whether it was anticipated and deemed as enforceable by the market. Following Ali and Kallapur (2001), we estimate the market model to examine abnormal returns to a portfolio of 20 firms that investors can sue according to the Notice. Although the stock returns were not significant in any of the four weeks before the issuance of the Notice, there was a significantly negative return of about 5.4 percent (bootstrap t-statistics = −3.218) for the portfolio during the Notice week.[22] The evidence indicates that the Notice was indeed a surprise to the market. Moreover, the Notice was perceived to be effectual: the market discounts affected firms' stock prices because the impending lawsuits could result in the transfer of wealth from the defendant companies to the plaintiffs. We therefore believe that it is appropriate to treat 2001 as the first fiscal year to be affected by institutional changes.

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The Interdependence between Institutional Ownership and Information Dissemination by Data Aggregators

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ABSTRACT: This study examines the interdependence between institutional ownership and the speed with which Standard & Poor's disseminates corporate accounting information. From the demand-side perspective, we find that quasi-indexers, who rely on corporate accounting information as a low-cost monitoring system, are the key driver of the institutional demand for speedy information dissemination. In addition, dissemination speed increases substantially for stocks listed in major market indices but decreases with high arbitrage risk or transaction costs. From the consequences perspective, we find that both transient investors and quasi-indexers gravitate to stocks with faster information dissemination, consistent with the latter using accounting information as a low-cost performance-monitoring mechanism, and the former being better enabled to implement their trading strategies in a richer information environment. Overall, this study provides new insights into the capital market information infrastructure by examining how information intermediaries and sophisticated investors impact each others' resource allocation decisions.

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> The acquisition of information and its dissemination to other economic units are, as we all know, central activities in all areas of finance, and especially so in capital markets.
>
> —Merton (1987, 485)

I. INTRODUCTION

This study examines the interrelationship between institutional investor stock ownership and the dissemination speed of key corporate accounting information by Standard & Poor's (S&P) Compustat. The key role of data aggregators in capital markets is highlighted in the following quote from PricewaterhouseCoopers' letter to the SEC (June 8, 2006) on the use of XBRL:

> based on our discussions with investors and analysts, we understand that investors acquire the large majority of relevant information, including financial data, from sources not controlled by the reporting entity. These information intermediaries employ automated parsing technologies and manual resources to parse public filings and other data sources into standardized and electronically reusable data. (http://www.sec.gov/news/press/4-515/4515-8.pdf)

Academic research and anecdotal evidence show important economic links between a firm's informational characteristics, including its disclosure practices, and key traits such as cost of capital, investor clientele, and stock price volatility (see, for instance, Potter 1992; Sias 1996; Botosan 1997; Fox 1997; Serwer 1997, Healy et al. 1999; Bushee and Noe 2000). However, there is no substantial body of research that examines the role that data aggregators play in providing readily accessible corporate financial data in standardized format, facilitating valuation and analysis, and potentially affecting perceived information risk. We address this gap by studying the information dissemination practices of Standard & Poor's, an important information intermediary that acts as a key supplier in the market for corporate accounting information.

The sample for our study consists of 319,524 firm-quarters during the period 1991–2004 for which we have S&P's data collection lags, measured as the difference between the periodic SEC filing dates and the corresponding Compustat Research Insight production dates when information from SEC filings is disseminated by S&P to its commercial customers. Our first set of analyses examines the extent to which S&P's data collection lags are determined by the strength of the demand the data aggregator faces for corporate accounting information. We identify institutional investors as the key source of demand for the services of capital market data aggregators, and find that while Standard & Poor's firm-specific data collection lag is decreasing in the firm's overall level of institutional ownership, it is also influenced by the nature of institutional investor clientele. Bushee (1998, 2001) identifies three types of institutional investors by trading behavior: transient investors are characterized by high diversification, high turnover, and short horizons; dedicated investors are characterized by "relation investing" in a few selected firms, with low turnover and long horizons; and quasi-indexers are characterized by high diversification, but low turnover. We predict and find that the relationship between institutional ownership and dissemination lag is driven by the informational demands of quasi-indexers. These results are consistent with Bushee and Noe (2000), who argue that quasi-indexers depend on corporate financial disclosures as a low-cost performance-monitoring mechanism, while neither transient nor dedicated institutions rely on quick access to standardized corporate accounting information to meet their investment objectives.

We also examine several investment characteristics that influence the demand for speedy dissemination of corporate information. Given that high arbitrage risk and transaction costs deter sophisticated investors from exploiting accounting-based mispricing (see supporting evidence in Collins et al. 2003; Ali et al. 2003; Mashruwala et al. 2006), demand for corporate accounting information is likely to be lower when arbitrage costs are high. Consistent with this reasoning, we predict and find less speedy dissemination of corporate accounting information when stocks have higher arbitrage risk and transaction costs.

Due to the heightened investor awareness for index stocks, we predict greater demand from institutional equity clients for information on stocks included in major market indices, and more so for large cap indices compared to middle or small cap indices. Results are consistent with hypothesized collection priorities, documenting a 41 percent faster collection of information from periodic SEC filings for companies in the S&P 500 index, with market capitalization and memberships in other domestic major market indices having smaller impacts. We also find that membership in a major index (or the factors that lead to it) may be more important to trigger investor awareness and demand for information than merely large market capitalization.

While our hypotheses pertain to the demand side, we control for various supply-side factors likely to influence collection lags: production-capacity constraints, peak-period shifts in S&P's resources, firm-specific differences in collection efforts, and enhanced technological efficiencies. We find higher collection lags during peak times, consistent with limits to production capacity and the inability to "stock" information in anticipation of demand. We also find that the collection lag is concave in the magnitude of the backlog, consistent with the notion that S&P deploys increased resources during peak periods. In addition, we document that the collection lag increases with data aggregation complexity for a firm's periodic report. Finally, the supply shock introduced by EDGAR has resulted in roughly a 50 percent reduction in the average collection lag. Collectively, our demand and supply factors explain a significant cross-sectional variation in S&P's data collection lag.

Our second set of analyses focuses on the impact of information dissemination speed on the trading decisions of various types of institutional investors. Based on extant research (e.g., Bushee and Noe 2000), we predict that transient investors are more likely to be drawn to firms with faster dissemination of corporate accounting data, given that a richer information environment facilitates the implementation of their short-run trading strategies. As financial disclosures offer a low-cost monitoring mechanism for quasi-indexers, we predict that quasi-indexers will also be drawn to firms with faster information dissemination speeds. Bushee and Noe (2000) argue that dedicated investors have longer horizons and invest in a few selected firms and are likely to be indifferent to the liquidity benefits of a richer information environment. However, there is evidence that dedicated investors who have longer horizons gravitate to investments characterized by higher information asymmetry (see Wang and Zhang 2009), consistent with incentives to retain their informational advantage. Given the ambiguity in the expected behavior, we make no directional predictions about the effect of dissemination speed on the trading decisions of dedicated investors.

We test the impact of dissemination speed on institutional ownership by analyzing the quarter-to-quarter changes in ownership by transient investors and quasi-indexers as a function of the speed with which corporate accounting information is disseminated by S&P,

after controlling for various determinants of changes in institutional ownership used in past research (Ke and Petroni 2004; Ke and Ramalingegowda 2005). For both transient investors and quasi-indexers, results are consistent with predictions: these institutional investors increase (decrease) their holdings as information dissemination speed increases (decreases). Although we posit no specific predictions for dedicated investors, results indicate that these investors increase their ownership position when dissemination of data from periodic filings is delayed, consistent with arguments in extant research that they prefer coarser information environments.

Taken together, our findings should be of interest to academic researchers, the investment community, and other market participants. Our study takes a first look at factors influencing the speed of dissemination of corporate accounting information by data aggregators, and provides evidence on the mutual effects that data aggregators and institutional investors have on each others' resource allocation decisions.

Section II provides a brief description of the theory regarding the market for information goods and discusses our hypotheses. Section III discusses sample selection and descriptive evidence. Section IV examines the determinants of the dissemination speed of corporate accounting information, followed by an analysis of the consequences of dissemination speed in Section V. The results of additional sensitivity analyses are discussed in Section VI. Concluding remarks are provided in the final section.

II. THEORY AND HYPOTHESES

In this section we first describe the unique characteristics of the market for corporate accounting information, with special focus on the commercial market segment served by S&P's Compustat. We conclude this section with a discussion of our hypotheses.

Market for Information Goods: The Case of Corporate Accounting Information

Consistent with the reasoning in Shapiro and Varian (1999), the market for information goods faces an oligopolistic competition with a handful of large players providing differentiated products and services.[1] Capital market data aggregators segment the market at a minimum between commercial and educational customers, who are provided different product versions with different timeliness for which they are charged different prices.

The main subscribers of Compustat's commercial database include buy-side hedge funds, large and small money managers, equity and credit analysts, and corporations.[2] To encourage broader commercial appeal, Compustat charges subscription fees based on assets under management.[3] Our discussions with S&P and a large money manager suggest that institutional investors who engage in sophisticated statistical analysis of accounting information for their investment strategies (akin to "quasi-indexers") require high-quality standardized data that are not only comprehensive in the cross-section, but also have a long time-series. A comprehensive coverage of public companies gives Compustat an advantage in assisting money managers to develop unbiased strategies. The long time-series is especially attractive to money managers who need to back test using historical data for any new investment strategies they develop.

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[1] We reviewed the periodic filings of a pure play entity in the market for corporate financial information (i.e., FactSet) to identify key competitors in this market. The 2007 10-K report of FactSet identifies only three corporate financial data aggregators as competitors: Bloomberg, S&P, and Thomson-Reuters. Consistent with FactSet's description of the competition, our discussions with a large mutual fund indicate that, for purposes of quantitative analysis, they rely on Compustat, Worldscope (Thomson), I/B/E/S (Thomson), and Ford Equity Research for corporate financial information (with CRSP acting as the primary source for stock market data). As of now, this market has not been significantly impacted by the instant availability of data through electronic tagging made possible by EDGAR. Consequently, data aggregators such as Edgar Online and 10-K Wizard do not yet appear to be direct competitors of S&P, Bloomberg, and Thomson-Reuters.

[2] The discussion that follows is based on four steps we took to learn about the demand for Compustat's products and services: (1) review of S&P's marketing brochures and pamphlets; (2) participation in a WebEx session offered by S&P to potential subscribers; (3) a phone interview with a Compustat staff member; and (4) various email exchanges with Compustat staff.

While instantaneous access is not universally demanded, a vital attribute of each aggregator's perceived service quality is likely to be the speed with which it provides information most in demand by its customer base. Because data aggregators cannot "stock" information in anticipation of customer demand, they build up costly production capacity[4] to cater to both client-specific requests for expedited information on individual companies and the general demand from their institutional equity clients. Therefore, we argue that a key choice variable for data aggregators is the production capacity deployment strategies as reflected in their firm-specific dissemination speed choices. Overall, information dissemination speed is an equilibrium choice reflecting both customer needs (demand) and data aggregators' production capacity choices (supply).

Hypotheses

In the following two subsections we develop hypotheses about the influence of institutional investors on the speed of information dissemination by S&P as well as the consequences of the dissemination speed on institutional ownership.

The Influence of Institutional Investors on Dissemination Speed

As discussed earlier, institutional investors (especially money managers) are the key commercial subscribers to Compustat's corporate accounting databases, which suggests that stocks held by institutional owners will receive higher priority in general. In addition, our discussions with Compustat indicate that special requests by clients receive the *highest* data collection priority, and Compustat expedites data collection efforts in such cases.[5] Such special requests are likely to further speed up information dissemination for stocks typically held by institutions.[6] Assuming that Compustat's collection efforts are geared toward meeting its customer needs, we predict the following (all hypotheses stated in alternative form):

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[3] This pricing scheme is consistent with the increasing value of information as a function of assets under management. Given that the differential commercial price arrangements used by Compustat do not lead to different timeliness of data delivery among commercial customers, we view commercial customers as one market segment for purposes of this study. We are not aware of any usage-based fees charged by Compustat to its commercial customers, which is consistent with Sundararajan (2004) that such pricing strategy can be less than optimal when transaction costs of administering is non-trivial.

[4] For instance, to robustly convert the nonstandard formats in which corporate accounting data are provided into a standardized electronic database, S&P's Compustat hires data collectors with accounting expertise and trains these professionals in detailed standardization and coding procedures. The Compustat User's Guide is more than 700 pages long, with detailed explanations of the specific information that is included in or excluded from each data item.

[5] See also http://www2.standardandpoors.com/spf/pdf/products/Compustat2006.pdf (accessed February 2007).

[6] We find several instances in the portfolio holdings disclosure policy section of registration statements where mutual funds indicate that they provide access to information on portfolio holdings to data aggregators such as Standard & Poor's and Vestek. Examples include the registration statements of Pacific Select Fund, Janus Capital Management, Forward Funds, The Roxbury Funds, and WT Mutual Fund. These disclosures are consistent with S&P's statement that its clients request more timely data gathering on selected companies at selected times.

H1: There is a negative association between institutional ownership and the delay in the dissemination of corporate accounting information.

Bushee and Noe (2000, 176) posit that quasi-indexers prefer firms with forthcoming disclosures, as they offer "a cost-effective method of monitoring firm performance." Consistent with this argument, they find that quasi-indexers gravitate more toward stocks of firms with higher analyst ratings of their mandatory disclosure practices than toward those with stronger investor relationship programs. Given that quasi-indexers appear to depend on corporate accounting disclosures as a low-cost performance monitoring mechanism, S&P is likely to expedite its data collection efforts for stocks favored by them.

Porter (1992) and Dobrzynski (1993) provide evidence that dedicated institutions follow a relationship strategy with low portfolio turnover and large stakes in the smaller group of stocks they follow. In addition, they rely less on public disclosures, as they have better access to private information about their portfolio firms (see Porter 1992; Bushee and Noe 2000). Not only is public disclosure less important for their monitoring activities, but dedicated institutions might even consider them "potentially costly if it reveals proprietary information" (Bushee and Noe 2000, 176). As dedicated investors rely less on public channels of communication, data aggregators are likely to place a lower priority on stocks held by these institutional investors.

While the trading decisions of transient institutions could be indirectly affected by the dissemination speed (see the subsection for investor response predictions), Bushee and Noe (2000) argue that these institutions do not necessarily *rely* on the accounting information provided by information intermediaries, which, in turn, suggests that they are unlikely to be a significant source of demand for firm-specific information dissemination speed. Overall, data aggregators are more likely to focus on collecting accounting information of firms targeted by quasi-indexers. Consequently, we expect the level of holdings of quasi-indexers alone to impact dissemination speed, and hypothesize that:

H2: There is a negative (no) association between quasi-indexer (transient/dedicated) institutional ownership and the delay in the dissemination of corporate accounting information.[7]

While H1 and H2 focus on the investor characteristics, we turn next to stock-specific characteristics that influence institutional investors' general demand and special requests for information. Collins et al. (2003) argue that sophisticated market participants, such as institutional investors, are unable to benefit from mispricing opportunities due to arbitrage risk and transaction costs (Ali et al. 2003; Mashruwala et al. 2006). Therefore, we argue that the institutional investor demand for corporate accounting information is likely to be greater when stocks have lower arbitrage risk or transaction costs, leading to the following hypothesis:

H3a: The delay in the dissemination of corporate accounting information is lower for stocks with lower arbitrage risk or transaction costs.

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[7] We interviewed the chief operating officer and a member of the quantitative research team of an international investment management firm with client assets close to $150 billion. Our discussions with them are entirely consistent with H2. Their quantitative research portfolio (akin to quasi-indexers) staff employs data-driven statistical analysis that relies heavily on publicly available aggregated data. On the other hand, their relational investment ("dedicated investor") portfolio staff closely monitors companies' financial performance (including reviewing filings, press releases, other market information, meeting with management, etc.), but do not rely on standardized panel data provided by data aggregators.

The findings of Wurgler and Zhuravskaya (2002) and Chen et al. (2004) suggest that another indication of demand for a stock held by institutional investors is its membership in a major market index.[8] Therefore, we expect greater demand for corporate accounting information of stocks included in major market indices, and more so for stocks included in large cap indices compared to medium or small cap indices.[9] Our discussions with Compustat indicate that S&P's data collection priorities are designed to reflect the greater demand for information on more visible stocks, which leads us to predict the following:

H3b: The marginal reduction in the dissemination lag of corporate accounting information is the highest for stocks included in the S&P 500 index, followed by smaller effects for stocks with market capitalization of $1 billion or more, and stocks in other S&P domestic major indices (S&P MidCap 400 and S&P SmallCap 600).[10]

The Institutional Investor Response to Dissemination Speed

Our final hypothesis focuses on the impact of dissemination speed on the trading decisions of the three types of institutional investors. Bushee and Noe (2000) find that transient institutions invest more heavily in stocks with a richer information environment (proxied by higher AIMR disclosure rankings), and add to their holdings in response to increases in disclosure rankings. They suggest that transient investors value "more forthcoming disclosure practices because such practices lessen the price impact of trades, facilitating the realization of short-term trading gains" (Bushee and Noe 2000, 173). In other words, a finer information environment preserves the liquidity in the market for a stock, which is "likely the key benefit of disclosure for transient institutions" (Bushee and Noe 2000, 187). A key insight from Bushee and Noe (2000) is that while transient investors may not necessarily rely on the accounting information provided by information intermediaries, their trading decisions are likely to be influenced by the dissemination speed to the extent it impacts market liquidity. Therefore, we predict that transient investors are more likely to be drawn to firms whose information environment is enhanced by faster information dissemination, which would facilitate the implementation of their short-run trading strategies.

The results in Bushee and Noe (2000, 187) suggest that "quasi-indexers are more sensitive to the informativeness of mandatory reports than to investor relations activities, which is consistent with their fragmented ownership positions and reliance on corporate disclosures to monitor firm performance." Given that corporate financial disclosures offer a low-cost monitoring mechanism for quasi-indexers, we also predict that quasi-indexers will be drawn to firms with faster information dissemination speed. This provides the rationale for our prediction that quasi-indexers both *demand* faster dissemination of corporate accounting disclosures (H2) as well as *gravitate* toward stocks whose accounting information is disseminated quickly. Unlike transient investors, quasi-indexers prefer a finer information environment for the sake of the information itself rather than for higher market liquidity.

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[8] Wurgler and Zhuravskaya (2002) find that stocks added to the S&P 500 index experience positive demand shocks, especially when they are part of large index fund asset portfolios. Consistent with the investor awareness argument, Chen et al. (2004) report a permanent increase in the price of stocks added to the S&P 500 index with no permanent decline for deleted firms.

[9] See Bos and Ruotolo (2000) for the criteria for inclusion of a stock in one of the S&P domestic indices.

[10] Compustat staff indicated that their data collection and processing priority is as follows: client requests, S&P 500, TSE 300, billion dollar market cap, remaining S&P 400/600, remaining Russell 3000, and low priority. While S&P collection priorities may have evolved over time, H3b captures their essence in more recent times. We do not create a separate category for remaining Russell 3000 stocks or stocks with less than or equal to $250 million, so their collection priority is reflected in the intercept. For sake of parsimony, we also do not create a separate category for TSE 300, which represents just over 1.5 percent of sample firm-quarters. In addition, 61 percent of these firm-quarters are in the S&P 1500 index (including S&P LargeCap 500, MidCap 400 and SmallCap 600) or have market capitalization greater than $1 billion, so most TSE firms would be represented in the categories described in H3b. While the indicated collection priority is relevant during our sample period, it appears that Compustat has since shifted the data collection priority of S&P 400/600 firms above that of firms with market capitalization greater than $1 billion (http://www2.standardandpoors.com/spf/pdf/products/Compustat2006.pdf).

Using equity analyst ratings as a proxy for public disclosure quality, Bushee and Noe (2000, 173) find that "dedicated institutions ... show no sensitivity to disclosure rating levels or changes, suggesting that corporate disclosure practices are not a significant factor affecting these institutions' investment decisions." In addition, due to their aversion to frequent trading, dedicated institutions are unlikely to value the liquidity benefits of a finer information environment (Bushee and Noe 2000, 176). They are less dependent on public channels of communication, and might arguably even prefer disclosure environments that enable them to retain their informational advantages. Consistent with this possibility, Wang and Zhang (2009) find that the equity holdings of dedicated institutions "are positively associated with indicators for information asymmetry." The implication in the literature is that these institutions are long-term focused, therefore they are less interested in short-term price movements, but expect to benefit from the market "ultimately" recognizing the value of their holdings. Given the ambiguity in the expected response, we make no directional predictions about the effect of dissemination speed on the trading decisions of dedicated investors, and restrict our hypothesis to transient and quasi-indexers as follows:

H4: Current changes in transient and quasi-indexer ownership are negatively associated with current corporate accounting information dissemination lag.

III. SAMPLE AND DESCRIPTIVE EVIDENCE ON THE SPEED OF INFORMATION DISSEMINATION

In this section, we first describe our sample screening criteria, and then provide descriptive statistics on S&P's information dissemination lag.

Sample Identification

Our sample selection procedure is summarized in Panel A of Table 1. Given that we examine information diffusion by capital market intermediaries, we focus on publicly traded firms in the CRSP database, and obtain our sample from the intersection of CRSP, the quarterly Compustat Preliminary History (Prelim) database, and the quarterly Compustat "As First Reported" (AFR) database. The table also details the reasons for loss of observations when merging with the S&P's SEC filing date database (which is necessary to calculate information dissemination lag). Based on the above considerations, we begin with a sample of 329,048 firm-quarters during the calendar years 1991 through 2004. We next exclude observations with missing production dates for the delivery of SEC filing information, or with collection lags greater than the 99th percentile. A small set of observations is dropped due to possible data coding problems. The resulting SEC filing sample includes 319,524 firm-quarters.

Institutional Details on Compustat Data Collection Practices and Measurement of Collection Lags

We use the AFR database production dates for the S&P's Research Insight product to examine the speed with which S&P disseminates accounting information to the marketplace. The relevant production date variable is *FINALQPRD*, which represents the production date

TABLE 1
Sample Selection and Descriptive Statistics on Compustat Data Collection Lags

Panel A: Sample for Analysis of Periodic SEC Filings Collection Speed

Firm-quarters in Compustat Preliminary History, Compustat AFR database, and CRSP over 1991–2004	401,707
Reasons for Missing Filing Date:	
Foreign firms that may not file 10-K/10-Qs	(36,604)
Firms did not file 10-K/10-Q and Compustat did not collect data (missing *FINALQPRD*)	(1,818)
Firms did not file 10-K/10-Q and Compustat collected data from sources other than periodic filings[†]	(2,816)
Wholly owned subsidiaries of publicly traded firms,[‡] subsidiaries without publicly traded common stock,[#] or subsidiaries not traded on NYSE, AMEX, NASDAQ or a regional exchange[§]	(3,440)
Firms trading pink sheets or over-the-counter bulletin board[$]	(3,047)
Divisions of merged firms that file separate financial reports[*]	(27)
Firm-quarters with missing SEC filing date with unexplored reasons (e.g., certain regulated firms, firms undergoing merger and acquisitions, firms changing names, etc.)	(24,907)
Firm-quarters in Compustat preliminary history, Compustat AFR database, CRSP, and SEC filing date database over 1991–2004	329,048
Less:	
Firm-quarters with missing Compustat production date (*FINALQPRD*)	(4,664)
Filing date preceding fiscal quarter end	(49)
FINALQPRD preceding filing date	(1,594)
Firm-Quarters with Compustat collection lag longer than 99 percentile (129 weekdays)	(3,217)
Final sample	319,524

Panel B: Descriptive Statistics on S&P Dissemination Lag of Periodic SEC Filings ($DL_{iq,tp}$)

	n	Mean	Std	Min	Q1	Median	Q3	Max
All	319,524	18.437	15.562	0	8	15	23	129
Pre-EDGAR (1991–1995)[¥]	97,741	24.696	16.147	0	16	22	29	129
Post-EDGAR (1997–2004)[¥]	195,707	14.116	12.640	0	7	11	18	129

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[*] The ticker of the merged firms is followed by a ".CM."

[†] Other source documents include no source document prior to data collection (00), *Wall Street Journal* (02), quarterly report (03), uniform statistical report (04), quarterly report from summary annual report (09), quarterly report and quarterly supplement (15), Newswire (20), news releases (30), quarterly report and news release (33), prospectus (37), quarterly report and uniform statistical report (43), subsequent period source (88), and no source documents due to gap in data collection (99).

[‡] Wholly owned subsidiaries of publicly traded companies have special Compustat ticker symbol comprised of the parent company's ticker followed by a number that identifies a particular subsidiary of that parent.

[#] Compustat assigns ticker symbols that consist of four digits followed by an "A" to such subsidiaries.

[§] Compustat assigns ticker symbols that consist of four digits followed by a "B" to such subsidiaries.

[$] Compustat precedes those firms' ticker with the number "3."

[¥] Although EDGAR became effective on May 6, 1996, our discussions with S&P indicate that Compustat had not obtained the full benefits of EDGAR in the year of transition. Therefore we eliminate year 1996 (26,076 firm-quarters) from both pre- and post-EDGAR periods.

Panel A of this table describes our sample selection process which yields 319,524 firm-quarters in the final sample. However, the actual sample size varies with financial data availability in the tables reporting regression results. Panel B reports the summary statistics of Compustat collection lag of SEC 10-K/10-Q filings over 1991–2004 and separately for pre- and post-EDGAR periods. *DL* is measured as the number of weekdays from a company's 10-K/10-Q filing date to Compustat production date when the company's finalized financial data first appears in Compustat (*FINALQPRD*).

when a company's final quarterly financial data from periodic SEC filings first appeared in Compustat.[11] We calculate dissemination lag (*DL*) as the number of weekdays between the SEC periodic report filing date and *FINALQPRD*.

Panel B of Table 1 indicates that the median *DL* is 15 weekdays (equal to Compustat's timeliness standard for AFR data during peak season), with the inter-quartile value ranging from 8 to 23 weekdays.[12] We also provide descriptive statistics on *DL* separately for the pre-EDGAR (1991–1995) and post-EDGAR (1997–2004) periods. A comparison of median values indicates that EDGAR decreased the dissemination lag by 50 percent. Interestingly, the coefficient of variation increased from 0.65 (16.147/24.696) to 0.90 (12.640/14.116) after EDGAR, which suggests a large cross-sectional variation in dissemination speed even in the electronic filing era.

Compustat's Collection Efforts by Calendar Time

In Figure 1, Panel A we plot the average number of SEC filings (*#FL*) and "final updates" (*#FL_UPDATE*) (Compustat parlance for collection of data from 10-K/10-Q filings). The four large spikes in the arrival of periodic SEC filings correspond to the mandatory filing dates of calendar-year registrants in the pre-accelerated filing era (see Griffin 2003). When compared to the arrival of information (or filing of periodic reports), S&P's collection efforts are more evenly distributed in calendar time. In Figure 1, Panel B, we plot the average cumulative proportion of SEC filings and their collection over the 52 production weeks. The cumulative proportion of collection efforts is closer to a diagonal line, representing smoother collection efforts in calendar time when compared to the release of SEC periodic reports. More importantly, the dissemination lag for periodic filings indicates the presence of capital market participants who value access, albeit delayed, to standardized electronic databases that provide comprehensive accounting information on a broad cross-section of publicly traded companies.

IV. DETERMINANTS OF THE SPEED OF INFORMATION DISSEMINATION: MODEL AND EMPIRICAL FINDINGS

Model Development

To test our hypotheses on the determinants of information dissemination speed, we estimate the following panel-data regression:[13]

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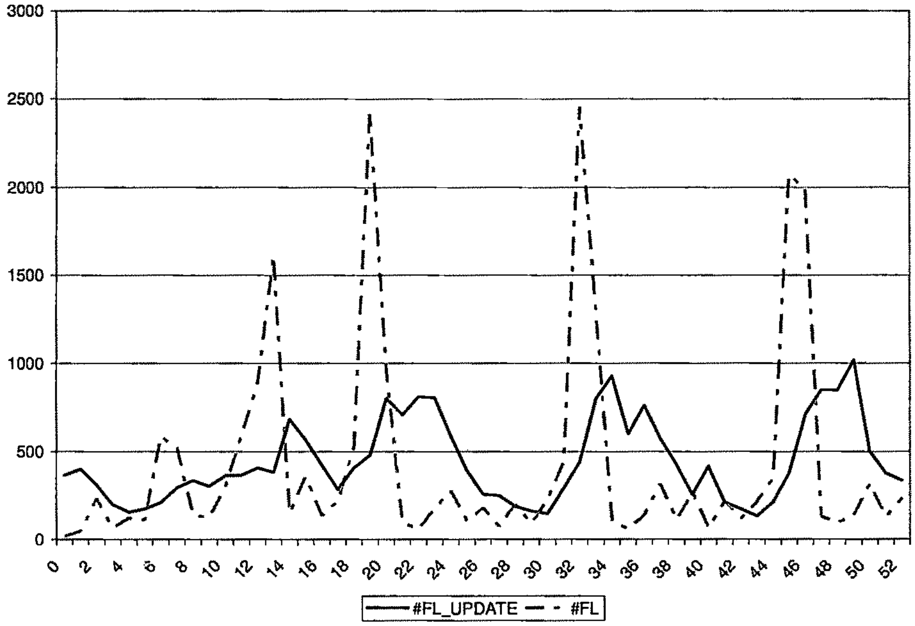
[11] See http://wrds.wharton.upenn.edu/ds/comp/manuals/dataguide/index.htm.

[12] *FINALQPRD* represents the Research Insight weekly CD-ROM production date, and therefore, could understate the true speed with which information is disseminated by Compustat to its commercial clients. Compustat offers its products using different delivery mechanisms with different dissemination speeds. For example, Compustat FTP and Xpressfeed rely on Internet-based delivery mechanisms that provide daily updates. Even customers receiving weekly CD-ROMs through their Research Insight subscriptions can seamlessly receive daily updates of key financial statement information. Consequently, the production date that we use may not represent the earliest point in time when data may be available to all of Compustat's commercial subscribers, giving rise to measurement error in our collection lag variables. However, note that *DL* extends beyond two production cycles for a majority of the firm-quarters, which suggests that the measurement error is less likely to be a concern.

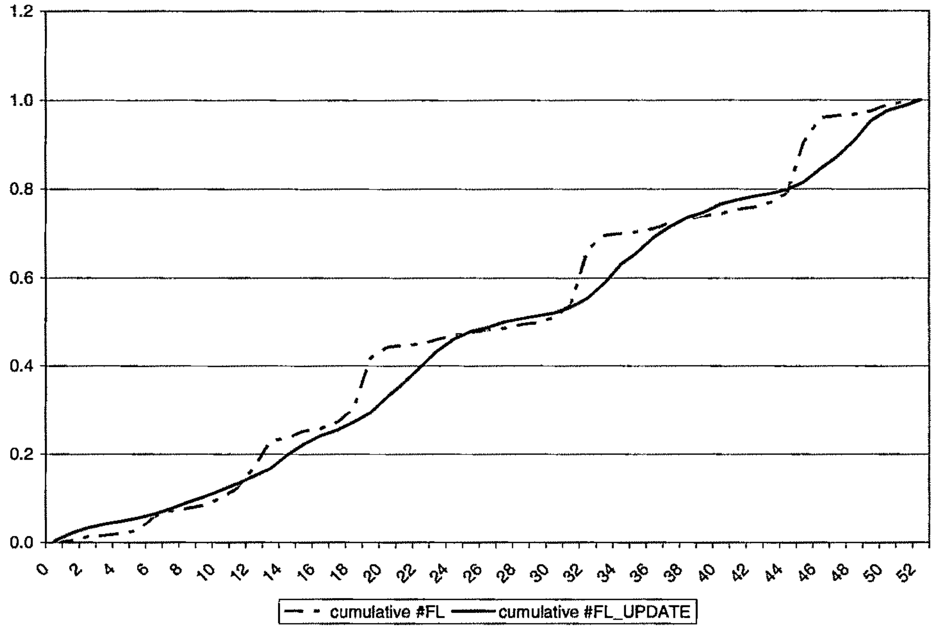
[13] While the focus of the paper is the dissemination of information in periodic SEC reports, we also examine the determinants of the dissemination speed of earnings press releases. Untabulated analysis show that the dissemination speed of earnings information has very limited cross-sectional variation, and the regression results show a pseudo R^2 of less than 4 percent. Given the importance of earnings announcements to the marketplace, the evidence indicates that S&P shifts its resources and collects earnings announcements within three business days in most cases, resulting in limited variation that can be explained by the posited determinants. However, many of the determinants of earnings announcement collection lags are statistically significant in the predicted directions. In the interest of parsimony, we restrict our discussion to the SEC filing regression results.

FIGURE 1
Intensity of Periodic SEC Filings and S&P's Collection Efforts

Panel A: Weekly Average Number of Releases of SEC Filings (*#FL*) and S&P's Collections (*#FL_UPDATE*), 1991–2004



Panel B: Weekly Average of Cumulative Proportion of Releases of 10-K/10-Q Filings (Cumulative *#FL*) and S&P's Collections (Cumulative *#FL_UPDATE*), 1991–2004



(*continued on next page*)

FIGURE 1 (continued)

The above figures compare the intensity of corporate financial information releases versus S&P data collection efforts over 1991–2004.

In Panel A, *#FL* is the weekly average number of SEC 10-K/10-Q filings, while *#FL_UPDATE* is the weekly average number of S&P's collections of SEC filings data, with both averages calculated using all weekly data over the sample period.

In Panel B, we divide the cumulative *#FL* by the aggregate number of final updates (solid line), and divide cumulative *#FL_UPDATE* by aggregate number of S&P's final updates (dashed line). Due to the measurement error in *#FL* at the beginning of the sample period, we exclude data in the first three month (January–March 1991). The x-axis represents the 52 weekly production cycles in a year.

$$\begin{aligned} DL_{iq,tp} = {} & \beta_0 + \beta_1 INST_{iq} + \beta_2 SPECTRUM_{iq} + \beta_{3a} ARBRISK_{iq} + \beta_{3b} PRICE_{iq} \\ & + \beta_{3c} VOLUME_{iq} + \beta_{4a} S\&P500_{iq} + \beta_{4b} CAP1B_{iq} + \beta_{4c} S\&P1000_{iq} \\ & + \delta_5 RATING_{iq} + \delta_{6a} FINSERV_i + \delta_{6b} UTILITY_i + \delta_{7a} SPECIAL_{iq} \\ & + \delta_{7b} EXTRAORD_{iq} + \delta_8 CONCUR_{iq} + \delta_{9a} EA\_BKLG_{t-1} \\ & + \delta_{9b}(EA\_BKLG_{t-1})^2 + \delta_{10a} FL\_BKLG_{t-1} + \delta_{10b}(FL\_BKLG_{t-1})^2 \\ & + \delta_{11a} \#EA_t + \delta_{11b}(\#EA_t)^2 + \delta_{12a} \#FL_t + \delta_{12b}(\#FL_t)^2 + \delta_{13} YREND_p \\ & + \delta_{14} QTREND_p + \delta_{15} MONDAY_p + \delta_{16} TUESDAY_p \\ & + \delta_{17} THURSDAY_p + \delta_{13} FRIDAY_p + \sum_{T=1992}^{2004} \beta_{19,T} YT_{iq} + \varepsilon_{iq,tp}. \end{aligned} \tag{1}$$

All variables are defined in the Appendix.

We use the overall percentage of institutional ownership (*INST*) in the first version of the model to test H1, and to break it down into equity ownership by transient investors, quasi-indexers, and dedicated investors in the second version to test H2. We obtain the institutional ownership data from CDA Spectrum, which extracts the data from the SEC's Form 13(f), available only on a calendar quarter basis.[14] Due to limited information on the institutional richness behind client information demand, the choice of the specific calendar quarter end to measure institutional holdings is not readily apparent. Consequently, we consider two different institutional ownership measurement dates to allow for different demand patterns, which are illustrated in Figure 2: (1) calendar quarter end at or immediately preceding the end of fiscal quarter q ($INST_q$); and (2) calendar quarter end at or immediately preceding the filing date of the periodic report ($INST_q^{Alt}$). We tabulate results based on $INST_q$, although our inferences hold with the alternative measurement date.

As in Mashruwala et al. (2006), we use stock price (*PRICE*) and dollar trading volume (*VOLUME*) as an inverse measure of transaction costs. Consistent with Mashruwala et al. (2006), Pontiff (1996), and Wurgler and Zhuravskaya (2002), we use idiosyncratic volatility (*ARBRISK*) as a proxy for arbitrage risk.

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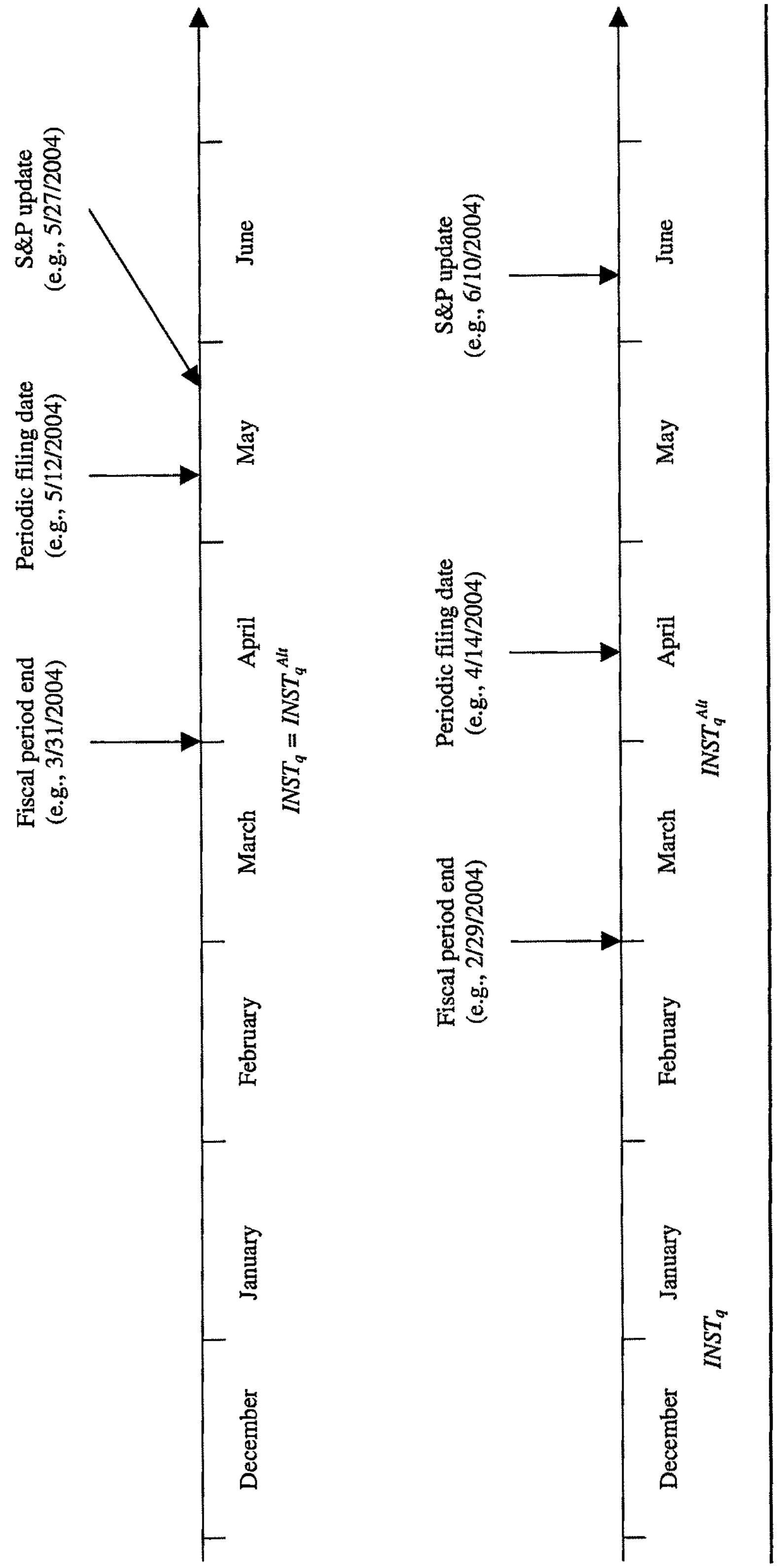
[14] Brian Bushee kindly provided the yearly classification of institutional investors into transient, quasi-indexers, and dedicated investors. We conduct a battery of robustness tests and find that Bushee's classification groups exhibit characteristics consistent with the posited investment styles. We also conduct a set of sensitivity analyses of the classification method on our hypothesis tests and find our inferences remain unchanged. Details of these analyses are available from the authors.

FIGURE 2
Time Line for S&P Data Collection


Fiscal period end (e.g., 3/31/2004)
Periodic filing date (e.g., 5/12/2004)
S&P update (e.g., 5/27/2004)
December
January
February
March
April
May
June
$INST_q = INST_q^{Alt}$
Fiscal period end (e.g., 2/29/2004)
Periodic filing date (e.g., 4/14/2004)
S&P update (e.g., 6/10/2004)
December
January
February
March
April
May
June
$INST_q$
$INST_q^{Alt}$

The regression model includes several control variables. Additional demand-side control variables include membership in a regulated industry (banks, insurance companies, and utilities), and the existence of S&P credit ratings.[15] We also control for supply factors that capture Compustat's capacity constraints as well as its resource allocation decisions in response to peak and non-peak demands. We use the current backlog of periodic reports in the S&P data collection system to proxy for the effect of capacity constraints, and incorporate the squared value of current backlog to capture increased Compustat resource allocation during peak times.[16] To examine any crossover effects of earnings announcement backlog on the dissemination speed of information in SEC filings, we include separate backlog variables for earning announcement and SEC filings. Intensity of information arrival is likely to influence dissemination speed, so we also control for the volume of earnings announcement and SEC filing information arrival. Similar to our backlog variables, we include both linear and quadratic terms for information arrival intensity. Finally, we control for the incidence of special or extraordinary items, as firms reporting these items are likely to experience business events that influence data aggregation complexity. To maintain its quality standards, Compustat is likely to take longer to collect data for such firms. We also include controls for instances in which earnings announcements and filing dates coincide, and calendar-year indicator variables to examine the supply-side effect of EDGAR on information dissemination.

Descriptive Statistics

We provide descriptive statistics on firm-specific variables in Panel A of Table 2, and on macro-level control variables in Panel B of Table 2. Descriptive evidence on institutional ownership and the proxies for arbitrage risk and transaction costs in Panel A is consistent with prior research (e.g., Collins et al. 2003; Mashruwala et al. 2006). Approximately 20 percent of the sample firm-quarters have an S&P credit rating (*RATING*), and slightly over 25 percent of the sample observations are included in Compustat's priority categories based on membership in major market indices or firm size (*S&P500 + CAP1B + S&P1000*). Regulated firms (*FINSERV + UTILITY*) comprise 22 percent of the firm-quarters, and slightly over 26 percent of sample firms report special or extraordinary items in their 10-Q/10-K filings (*SPECIAL + EXTRAORD*). The incidence of firms providing earnings information at the time of periodic SEC filings without a preliminary earnings announcement (*CONCUR*) is around 20 percent, which is similar to the incidence found in Amir and Livnat (2005) and Li and Ramesh (2009).

Panel B of Table 2 suggests that the time-series volatility in Compustat's collections efforts for earnings announcements (*#EA_UPDATE*) is similar to that of the arrival of earnings announcements (*#EA*), as indicated by their comparable standard deviations. In contrast, the time-series standard deviation in the arrival of periodic filings (*#FL*) is more than double that of S&P's collection efforts (*#FL_UPDATE*), indicating a smoother intertemporal collection effort for filings, and suggesting a relatively higher priority given to the collection of earnings announcements.

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[15] For financial services firms and utilities, users are likely to rely more on other readily available data sources that better reflect industry-specific idiosyncrasies than the standardized format used by Compustat. We control for firms with S&P credit ratings because S&P's credit-rating services rely on Compustat databases (private correspondence), and analysts and managers involved in credit evaluations constitute an important group of external customers as well.

[16] For example, some experienced Compustat data collectors do get promoted and are given professional opportunities outside its data collections group. However, Compustat finds it expedient to leverage the data collection expertise of these individuals during peak time periods. The institutional knowledge and expertise of these individual continue to be highly relevant for Compustat's data collection efforts.

TABLE 2
Descriptive Statistics

Panel A: Firm-Level Factors (n = number of firm-quarters)

	n	Mean	Std	Median	Min	Max	Q-Range
$INST_{i,q}$	256,900	33.877	26.651	28.928	0	100	44.594
$TRA_{i,q}$	256,900	10.023	12.090	5.421	0	99.781	14.614
$QIX_{i,q}$	256,900	15.895	14.356	12.072	0	100	20.928
$DED_{i,q}$	256,900	7.000	9.468	3.010	0	96.824	11.052
$SPECTRUM_{i,q}$	319,524	0.804	0.397	1	0	1	0
$ARBRISK_{i,q}$	308,802	3.941	2.791	3.190	0	19.460	3.046
$PRICE_{i,q}$	317,170	16.211	16.806	11	0.0001	102	19.183
$VOLUME_{i,q}$	309,120	6.808	25.269	0.358	0	235.778	2.415
$S\&P500_{i,q}$	319,524	0.074	0.262	0	0	1	0
$CAP1B_{i,q}$	315,524	0.096	0.295	0	0	1	0
$S\&P1000_{i,q}$	315,524	0.084	0.277	0	0	1	0
$RATING_{i,q}$	319,494	0.195	0.396	0	0	1	0
$FINSERV_{i}$	319,524	0.187	0.390	0	0	1	0
$UTILITY_{i}$	319,524	0.028	0.166	0	0	1	0
$SPECIAL_{i,q}$	319,524	0.221	0.415	0	0	1	0
$EXTRAORD_{i,q}$	319,524	0.043	0.203	0	0	1	0
$CONCUR_{i,q}$	309,614	0.201	0.401	0	0	1	0

Panel B: Macro-Level Factors (n = number of Compustat production cycles)

	n	Mean	Std	Median	Min	Max	Q-Range
$\#EA_{p}$	703	0.429	0.385	0.268	0.010	1.680	0.546
$\#FL_{p}$	703	0.451	0.781	0.188	0.010	4.481	0.268
$\#EA\_UPDATE_{p}$	703	0.429	0.384	0.268	0.012	2.050	0.538
$\#FL\_UPDATE_{p}$	703	0.451	0.367	0.341	0.022	2.425	0.430
$EA\_BKLG_{p-1}^{\dagger}$	703	0.233	0.216	0.152	0.013	1.263	0.234
$FL\_BKLG_{p-1}^{\dagger}$	703	1.713	1.307	1.223	0.120	7.069	1.732
$QTREND_{p}$	703	0.078	0.269	0	0	1	0
$YREND_{p}$	703	0.020	0.140	0	0	1	0

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† Given the backlog variables are likely to contain measurement error at the beginning of the sample period, we exclude the first three months' data in analyses (January–March 1991), but consider all data in the measurement of the backlog variables (beginning January 1991).

This table reports the number of observations, mean, median, standard deviation, minimum, maximum, and inter-quartile range (Q-Range) of the determinants of S&P information dissemination lag. Panel A reports summary statistics of firm-level variables. All variables except the last three in Panel A proxy for demand-side forces for corporate financial information. The last three variables in Panel A (*SPECIAL*, *EXTRAORD*, and *CONCUR*) and variables in Panel B, which reports summary statistics of macro-level variables, proxy for supply-side constraints. These variables are regressors in Table 3 and defined in the Appendix.

Regression Results

Table 3 shows the Poisson regression results of estimating model (1) using institutional ownership in the aggregate as well as broken down by investment style. Except when examining the institutional category effects, we limit our discussions to the first version of

TABLE 3
Poisson Regression of Compustat Data Collection Lag on Institutional Demand and Other Determinants[a]
(n = 293,577 firm-quarters)

This table reports the Poisson regression results where S&P collection lag is the dependent variable:

$$\begin{aligned} DL_{iq,tp} = {} & \beta_0 + \beta_1 INST_{iq} + \beta_2 SPECTRUM_{iq} + \beta_{3a} ARBRISK_{iq} + \beta_{3b} PRICE_{iq} \\ & + \beta_{3c} VOLUME_{iq} + \beta_{4a} S\&P500_{iq} + \beta_{4b} CAP1B_{iq} + \beta_{4c} S\&P1000_{iq} \\ & + \delta_5 RATING_{iq} + \delta_{6a} FINSERV_i + \delta_{6b} UTILITY_i + \delta_{7a} SPECIAL_{iq} \\ & + \delta_{7b} EXTRAORD_{iq} + \delta_8 CONCUR_{iq} + \delta_{9a} EA\_BKLG_{t-1} + \delta_{9b}(EA\_BKLG_{t-1})^2 \\ & + \delta_{10a} FL\_BKLG_{t-1} + \delta_{10b}(FL\_BKLG_{t-1})^2 + \delta_{11a} \#EA_t + \delta_{11b}(\#EA_t)^2 \\ & + \delta_{12a} \#FL_t + \delta_{12b}(\#FL_t)^2 + \delta_{13} YREND_p + \delta_{14} QTREND_p + \delta_{15} MONDAY_p \\ & + \delta_{16} TUESDAY_p + \delta_{17} THURSDAY_p + \delta_{18} FRIDAY_p + \sum_{T=1992}^{2004} \beta_{19,T} YT_{iq} + \varepsilon_{iq,tp}. \end{aligned}$$

		Regression 1			Regression 2		
	Pred. Sign	Coeff.	p-value	Marginal Effect	Coeff.	p-value	Marginal Effect
Demand-Side Factors							
$INST_{i,q}$	–	−0.0010	0.000***	−4.64%			
$TRA_{i,q}$	?				0.0004	0.044**	0.49%
$QIX_{i,q}$	–				−0.0030	0.000***	−6.22%
$DED_{i,q}$	?				0.0001	0.594	0.10%
$SPECTRUM_{i,q}$	?	−0.0024	0.663	−0.24%	0.0025	0.630	0.26%
$ARBRISK_{i,q}$	+	0.0024	0.001***	0.72%	0.0019	0.006***	0.58%
$PRICE_{i,q}$	–	−0.0007	0.000***	−1.27%	−0.0006	0.000***	−1.22%
$VOLUME_{i,q}$	–	−0.0007	0.000***	−0.16%	−0.0008	0.000***	−0.19%
$S\&P500_{i,q}$	–	−0.4126	0.000***	−41.26%	−0.3878	0.000***	−38.78%
$CAP1B_{i,q}$	–	−0.1276	0.000***	−12.76%	−0.1233	0.000***	−12.33%
$S\&P1000_{i,q}$	–	−0.2251	0.000***	−22.51%	−0.2131	0.000***	−21.31%
$RATING_{i,q}$	–	−0.0464	0.000***	−4.64%	−0.0432	0.000***	−4.32%
$FINSERV_i$	+	0.0878	0.000***	8.78%	0.0913	0.000***	9.13%
$UTILITY_i$	+	0.2197	0.000***	21.97%	0.2226	0.000***	22.26%
Supply-Side Factors							
$SPECIAL_{i,q}$	+	0.0176	0.000***	1.76%	0.0174	0.000***	1.74%
$EXTRAORD_{i,q}$	+	0.0717	0.000***	7.17%	0.0700	0.000***	7.00%
$CONCUR_{i,q}$		−0.0178	0.000***	−1.78%	−0.0184	0.000***	−1.84%
EA_BKLG_{p-1}		0.0407	0.018**	1.55%	0.0405	0.019**	1.54%
$(EA\_BKLG_{p-1})^2$		0.0116	0.264	0.36%	0.0120	0.249	0.37%
FL_BKLG_{p-1}		0.1831	0.000***	27.93%	0.1825	0.000***	27.84%
$(FL\_BKLG_{p-1})^2$		−0.0104	0.000***	−6.05%	−0.0103	0.000***	−5.99%
$\#EA_p$		0.3111	0.000***	3.83%	0.3156	0.000***	3.88%
$(\#EA_p)^2$		−0.9893	0.000***	−2.66%	−1.0020	0.000***	−2.70%
$\#FL_p$		0.1602	0.000***	18.07%	0.1592	0.000***	17.96%
$(\#FL_p)^2$		−0.0430	0.000***	−6.33%	−0.0427	0.000***	−6.28%
Year Effects							
Y1992		−0.1773	0.000***	−17.73%	−0.1744	0.000***	−17.44%
Y1993		−0.1204	0.000***	−12.04%	−0.1193	0.000***	−11.93%

(*continued on next page*)

TABLE 3 (continued)

	Pred. Sign	Regression 1 Coeff.	Regression 1 p-value	Regression 1 Marginal Effect	Regression 2 Coeff.	Regression 2 p-value	Regression 2 Marginal Effect
Y1994		−0.0779	0.768	−7.79%	−0.0764	0.000***	−7.64%
Y1995		0.0020	0.000***	0.20%	0.0022	0.747	0.22%
Y1996		−0.1207	0.000***	−12.07%	−0.1250	0.000***	−12.50%
Y1997		−0.6019	0.000***	−60.19%	−0.6078	0.000***	−60.78%
Y1998		−0.3250	0.000***	−32.50%	−0.3137	0.000***	−31.37%
Y1999		−0.3056	0.000***	−30.56%	−0.3059	0.000***	−30.59%
Y2000		−0.6333	0.000***	−63.33%	−0.6281	0.000***	−62.81%
Y2001		−0.7052	0.000***	−70.52%	−0.7181	0.000***	−71.81%
Y2002		−0.9306	0.000***	−93.06%	−0.9404	0.000***	−94.04%
Y2003		−0.8700	0.000***	−87.00%	−0.8669	0.000***	−86.69%
Y2004		−0.7401	0.000***	−74.01%	−0.7266	0.000***	−72.66%
Intercept		2.8049	0.000***		2.8061	0.000***	
Pseudo R^2		0.2273			0.2278		

*, **, *** Represent statistical significance at two-tailed 0.1, 0.05, and 0.01 levels, respectively.

[a] For brevity, we suppress the slope estimates of quarter-/year-end indicators and weekday indicators.

When data is not available in Spectrum, we set the institutional ownership variables ($INST_{i,q}$, $TRA_{i,q}$, $QIX_{i,q}$, $DED_{i,q}$) to 0 and include $SPECTRUM_{i,q}$ to control for lack of data availability from Spectrum. For each specification, we report estimated coefficients in the first column, followed by two-tailed p-value (based on Huber-White standard errors) in the second column, and the marginal effect in the third column. The marginal effect is the expected percentage change in the collection lag of SEC filings at the inter-quartile range for all continuous variables and for a unit change for all indicator variables.

the regression. We report Huber-White standard errors that are adjusted for heteroscedasticity and firm-specific clustering in our panel data (see Rogers 1993; Williams 2000; Petersen 2009).[17] We also report the marginal effects as the expected percentage change in the collection lag of SEC filings at the inter-quartile range for all continuous variables and for a unit change for all indicator variables.

The test and control variables explain a non-trivial variation in the dissemination speed as indicated by a 23 percent pseudo R^2. In addition, the results indicate that H1 and H2 are strongly supported. $INST_{i,q}$ (proxy for general demand and customers' special requests by institutional investors) is statistically significant in explaining Compustat collection efforts, with a marginal effect of 4.64 percent decline in dissemination lag. Moreover, when total institutional ownership is decomposed into the three categories of institutional investors discussed earlier, the result for institutional ownership in the first regression is driven by the significantly negative association between quasi-indexer ownership ($QIX_{i,q}$) and accounting information collection lag with a marginal effect of −6.22 percent, consistent with H2. The faster data collection by Compustat suggests that these investors place a premium on speedy access to accounting information in a comparable format for ease of performance monitoring and implementing their portfolio strategy. While dedicated investor ownership ($DED_{i,q}$) is not significantly associated with information dissemination speed, S&P collection efforts lag when transient ownership ($TRA_{i,q}$) increases, as indicated by the positive slope coefficient of 0.0004. Overall, neither transient nor dedicated investors appear to primarily rely on accounting information provided by data aggregators for their investment decisions.[18]

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[17] Our inferences hold when we include firm fixed effects with robust standard errors for the Poisson regression estimated per Wooldridge (1999).

Results also support the notion that stock-specific characteristics influence institutional demand for accounting information. The coefficients on *PRICE* and *VOLUME* are significantly negative, while the coefficient on *ARBRISK* is significantly positive, suggesting that demand for corporate accounting information is greater (and consequently collection speed is faster) when stocks have lower transaction costs or arbitrage risk, consistent with H3a.

Evidence strongly suggests that institutional investors demand faster dissemination of information on stocks included in major market indices or with large market capitalization, with membership in the S&P 500 index ($S\&P500_{i,q}$) resulting in the largest (41.26 percent) reduction in the dissemination lag, as expected. We find a 22.51 percent reduction in collection lag for stocks in the S&P MidCap 400 and S&P SmallCap 600 indices ($S\&P1000_{i,q}$). The collection lag for large firms ($CAP1B_{i,q}$) is also economically significant, with a marginal effect of 12.76 percent. But contrary to the collection hierarchy posited in H3b, the dissemination lag is shorter for firms in mid and small cap indices than for large firms.

In the interest of brevity, we discuss only selected control variables, although most controls are associated with collection lag in the expected direction. Consistent with dissemination speed being influenced by both external demand from credit managers and internal demand from S&P credit-rating service, we find that collection lags for firms with an S&P credit rating ($RATING_{i,q}$) are significantly lowered by 4.64 percent. We also find a significant delay in the dissemination of standardized corporate accounting information of regulated firms (*FINSERV* and *UTILITY*), possibly because of industry-specific information idiosyncrasies discussed earlier and/or lower demand for information on regulated firms. The effect is more salient for utilities, for which there is a 21.97 percent delay in the dissemination of information in periodic SEC filings.

Turning to supply-side effects, Compustat's collection lag increases by 7 percent for firms facing economic circumstances that lead to extraordinary accounting gains or losses (*EXTRAORD*), consistent with Compustat's increased efforts to meet its quality control standards in collecting complex accounting information. The effect of information complexity on dissemination lag is further corroborated by the significantly positive coefficient on *SPECIAL*. The slope coefficients of the linear and quadratic terms of the filing backlog variables have the correct sign and are statistically significant. The collection lag increases by over 27.93 percent (linear term) when the backlog of uncollected filing (FL_BKLG_{p-1}) data increases by its inter-quartile range, with the increased peak-time collection efforts bringing down the lag by roughly 6 percent (quadratic term). The data aggregator's response during peak times is consistent with Griffin's (2003) finding of a stronger market reaction to 10-K reports when filing intensity is high. Finally, our evidence shows a dramatic improvement in the dissemination speed of periodic filing information following the implementation of EDGAR, indicated by a large drop in its collection lag beginning in 1997 (*Y1997*).[19]

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[18] Our inferences are similar when we use log-transformed dollar value of institutional holdings as a measure of institutional demand, except that the slope for dedicated institutions is significantly negative although its magnitude is only one-tenth of that for quasi-indexers.

[19] Although EDGAR became effective on May 6, 1996 (Balsam et al. 2002), our discussions with S&P indicate that during 1996 Compustat was transitioning from the paper version of the filings to EDGAR, consistent with the EDGAR effect becoming more pronounced beginning in 1997.

Interactions between Investor Types and other Determinants of Dissemination Lag

We next consider the possibility of an interaction between investor types and other key determinants of dissemination lag, specifically the variables used to test H3a and H3b. First, we construct a combined measure of overall arbitrage costs, encompassing both arbitrage risk and transaction costs. We sort *ARBRISK* (ascending), *PRICE* (descending) and *VOLUME* (descending) into terciles ranked 0 to 2, and add the rankings to get a firm-specific combined score ranging from 0 to 6. We then create three indicator variables representing low, medium, or high arbitrage costs when the combined score is in the range 0–1, 2–4, or 5–6, respectively. We interact *TRA*, *QIX*, and *DED* with these three indicators and re-estimate the Table 3 regression. Interaction coefficient results are presented in Panel A of Table 4.

Consistent with sophisticated investors shunning stocks with high arbitrage costs (Collins et al. 2003; Ali et al. 2003; Mashruwala et al. 2006), we find that the interaction coefficients for quasi-indexers indicate a lower demand for speedy delivery of Compustat

TABLE 4

Poisson Regression of Compustat Data Collection Lag on the Interaction between Institutional Ownership and Stock Characteristics, and Other Determinants

(n = 293,577 firm-quarters)

Panel A: Interaction of Institutional Ownership and Arbitrage Cost

	Low Arbitrage Cost	Medium Arbitrage Cost	High Arbitrage Cost	High Minus Low
$TRA_{i,q}$	−0.0002	0.0008	−0.0008	−0.0006
	(0.603)	(0.004) ***	(0.083)*	(0.070)*
$QIX_{i,q}$	−0.0032	−0.0032	−0.0015	0.0017
	(0.000)***	(0.000)***	(0.000)***	(0.000)***
$DED_{i,q}$	0.0018	−0.0004	−0.0024	−0.0042
	(0.000)***	(0.128)	(0.000)***	(0.000)***

Panel B: Interaction of Institutional Ownership and Membership in Major Market Indices

	S&P500	*CAP1B*	*S&P1000*	*OTHER*	*OTHER − S&P500*
$TRA_{i,q}$	−0.0018	−0.0004	−0.0016	0.0012	0.0030
	(0.070)*	(0.356)	(0.001)***	(0.000)***	(0.003)***
$QIX_{i,q}$	−0.0054	−0.0045	−0.0034	−0.0020	0.0034
	(0.000)***	(0.000)***	(0.000)***	(0.000)***	(0.000)***
$DED_{i,q}$	0.0012	0.0011	0.0069	−0.0013	−0.0025
	(0.224)	(0.070)*	(0.000)***	(0.000)***	(0.011)**

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*, **, *** Represent statistical significance at two-tailed 0.1, 0.05, and 0.01 levels, respectively.

This table reports slope estimates from Poisson regressions of S&P collection lag on interactions of institutional ownership with arbitrage cost (Panel A) and with membership in major market indices (Panel B). All other control variables in Table 3 are also included in the regressions, but their slope estimates are suppressed for brevity. Two-tailed p-values (based on Huber-White standard errors) are provided in parentheses.

In Panel A, we first construct a combined measure for total arbitrage costs by ranking (from 0 to 2) *ARBRISK* (ascending), *PRICE* (descending), and *VOLUME* (descending) in terciles, and add up the rankings to get a combined score, which range from 0 to 6. We then create three indicator variables for low arbitrage costs for scores 0 and 1, medium arbitrage costs for scores 2 to 4, and high arbitrage costs for scores 5 and 6, respectively. We interact our institutional ownership variables with these three indicator variables.

In Panel B we interact institutional ownership with indicator variables representing the major stock market indices: *S&P500*, *CAP1B*, and *S&P1000*, as defined in Appendix. The firm-years where none of the three indicator variables take a value of 1 are assigned to *OTHER*.

data when arbitrage costs are high. The interactions do not show a consistent pattern for transient investors. For dedicated investors, the interaction coefficient is significantly positive for stocks with low arbitrage costs (0.0018), consistent with a lack of demand for information. However, for stocks with high arbitrage cost, the coefficient is significantly negative (−0.0024), suggesting that dedicated investors demand information on stocks with higher arbitrage costs and potentially higher information asymmetry, consistent with the findings of Wang and Zhang (2009).

We also interact institutional ownership measures with three indicator variables corresponding to membership in the S&P 500, billion dollar market cap, S&P 400/600 indices, and a fourth indicator for firms with no membership in any market index. The interaction coefficients are presented in Panel B of Table 4. Given the heightened investor awareness for index stocks (Wurgler and Zhuravskaya 2002; Chen et al. 2004), one would expect greater institutional demand for stocks included in major market indices. Results for quasi-indexers indicate that the collection lag is the shortest for S&P 500 firms (−0.0054), increases for stocks in less prominent indices (−0.0045 for *CAP1B* and −0.0034 *S&P1000*), and is the longest for stocks that do not belong to any major index (−0.0020). The results for transient investors do not lend themselves to an intuitive explanation. Similar to our results for arbitrage costs, we find evidence consistent with dedicated institutions demanding information on stocks with potentially lower investor awareness (slope of −0.0013 for *OTHER*).

Effect of Index Switching on Dissemination Speed

To the extent changes in investor awareness is asymmetric to additions versus deletions from major market indices, as documented in Chen et al. (2004), one would expect a corresponding asymmetric change in dissemination lag. To examine this relation, we expand the first version of the regression in Table 3 to incorporate indicator variables for switches among "index" memberships (i.e., two index groups and one firm-size group) and examine their marginal effect on information dissemination speed. Untabulated results indicate that there is a monotonic increase in the dissemination speed as we move from the lowest (S&P 1000) to the highest (S&P 500) priority group for firms that remain in the same priority group throughout our sample period. In terms of "index" switching, when firms are first added to the lowest priority index (S&P 1000), S&P's collection lag decreases by 14 percent with an additional decline of 17 percent when their market capitalization goes above $1 billion ("large firm"), and another 5.8 percent reduction when they move to the S&P 500. However, consistent with the investor-awareness literature, the dissemination speed increases for large firms only if their stock was first part of a major index. In terms of downward movements in priority, even when an S&P 500 firm ceases to have an "index" membership, its dissemination lag continues to be 13 percent shorter than other firms without an "index" membership (see Chen et al. 2004). Consistent with Shankar and Miller (2006), S&P does not retain any dissemination priority for stocks after they exit the S&P 1000 index.

V. CONSEQUENCES OF DISSEMINATION SPEED ON INSTITUTIONAL OWNERSHIP: MODEL AND EMPIRICAL FINDINGS

In the first subsection we provide descriptive evidence on the inter-temporal characteristics of firms that experience increasing or decreasing dissemination speed over our sample period. The results of our tests of H4 are discussed in the second subsection.

Descriptive Evidence on Inter-Temporal Changes in Dissemination Speed and Firm Characteristics

We first identity firms whose information dissemination *speed* has consistently increased over the 1991–2004 timeframe versus those for which the dissemination *lag* has increased during the same period. To insulate our analysis from the inter-temporal effects of EDGAR, we conduct a Z-transformation of *DL* of all firms following Blom (1958) by calendar year. Then, for each firm with at least eight quarters of data, we regress the Z-transformed *DL* on a time trend variable, assigning firms to the *SPEED* (*SLOW*) group if the coefficient on the time trend is significantly negative (positive) at a two-tailed p-value of 0.05. Unassigned firms are considered as having a stable inter-temporal dissemination speed. Overall, of the 10,279 firms in our panel data, we identify 1,319 (674) *SPEED* (*SLOW*) firms.

Figure 3 compares the dissemination speed and other firm characteristics between firms whose collection lags have trended upward versus downward over time. All values are reported as deviations from those of the "stable" firms. The directional evidence in Figure 3, Panel A shows, by construction, two sets of firms with divergent collection lags over the sample period. In terms of magnitude, the evidence shows an economically significant decline (increase) of around 10 (20) days in the collection lag of the *SPEED* (*SLOW*) group. Except for the S&P 1000 membership variable (Figure 3, Panel E), the time trend of other firm characteristics shown in Figures 3, Panels B through G is generally consistent with the information environment differences between the two groups. For instance, firms in the *SPEED* group have gained an additional 15 percent institutional ownership, as compared to the *SLOW* group (Figure 3, Panel B). Similarly, firms in the *SPEED* group have mustered roughly three additional equity analysts relative to the *SLOW* group (Figure 3, Panel G) with about 7 percent more firms receiving a credit rating (Figure 3, Panel F). The graphs for *S&P500* (Figure 3, Panel C) and *CAP1B* (Figure 3, Panel D) show that the increase in collection speed is associated with a greater likelihood of membership in the S&P 500 index or with increasing market capitalization.

Institutional Investor Response to Changes in Information Environment

Following Ke and Petroni (2004) (see also Ke and Ramalingegowda 2005), we estimate the following model to examine the predictions of H4 regarding the institutional investor response to dissemination speed:

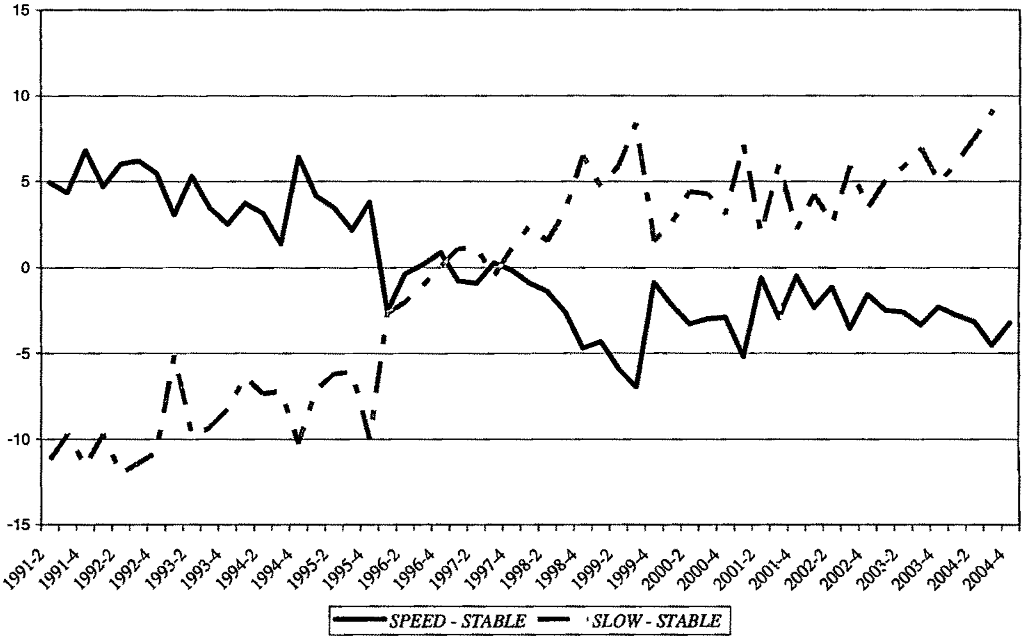
$$\begin{aligned}\Delta INST_{iq} &= \beta_0 + \beta_1 DL\_KNOWN_{iq} + \beta_2 INFOSTATUS_{iq} + \delta_3 OWN_{iq-1} \\ &\quad + \delta_4 PORTWT_{iq-1} + \delta_5 Ln(MVE_{iq-1}) + \delta_6 BTM_{iq-1} + \delta_7 RETQ24_{iq} \\ &\quad + \delta_8 RETQ1_{iq} + \delta_9 RETQ0_{iq} + \delta_{10} UE_{iq} + \varepsilon_{iq}. \qquad (2)\end{aligned}$$

The dependent variable in model (2) is the quarter-to-quarter change in the institutional ownership of a given firm. In terms of the hypothesized effect, we consider the actual dissemination lag (*DL*) of the most recent fiscal quarter relative to calendar quarter *q* as the variable of interest. Given the lack of an inter-temporal persistence in *DL* (untabulated analysis), we consider the *level* of the lagged dissemination speed as the predictor of changes in institutional ownership. Note, however, that for certain firms, the actual dissemination of corporate accounting information may not have occurred by the end of the current calendar quarter (*q*) because either the firms have not yet filed their periodic SEC reports or Standard & Poor's has not yet collected the information. We include an indicator variable

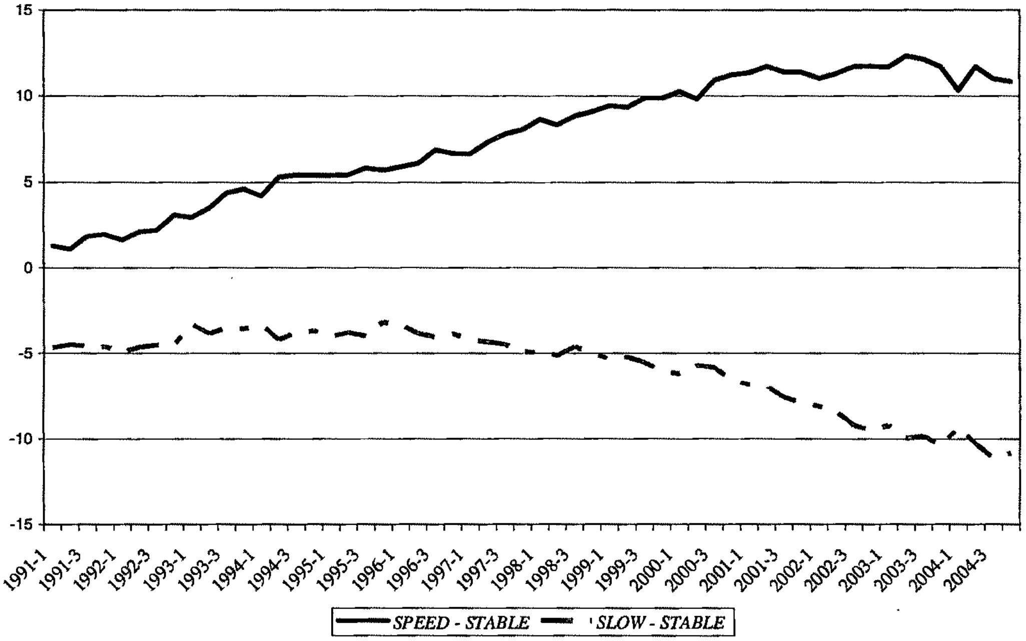
FIGURE 3

Inter-Temporal Trend of Selected Firm Characteristics Proxying for Information Environment

Panel A: Mean Difference in *DL*



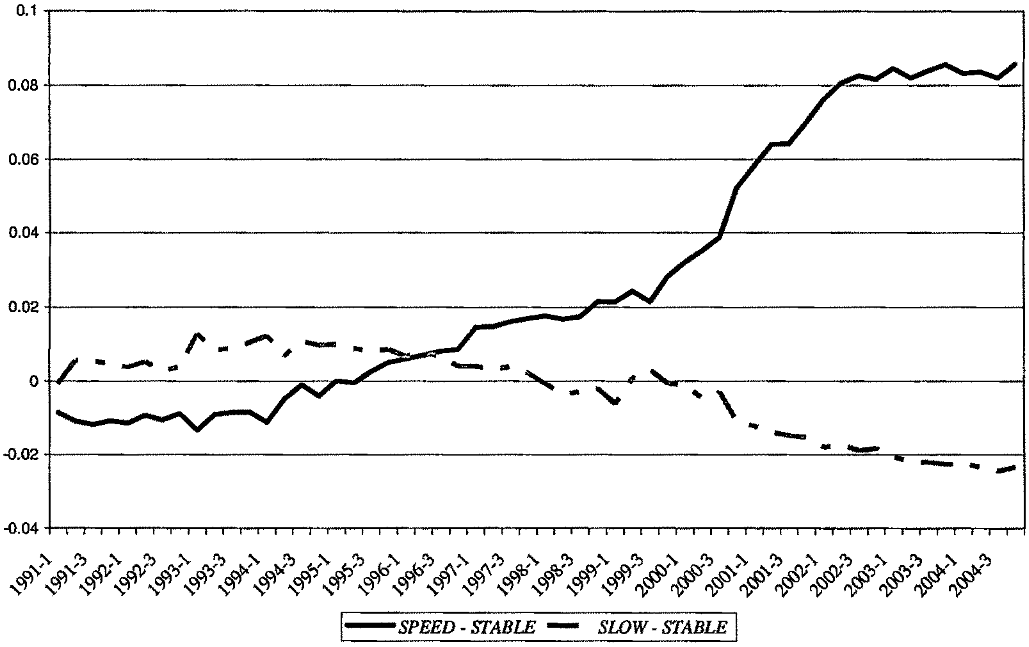
Panel B: Mean Difference in *INST*



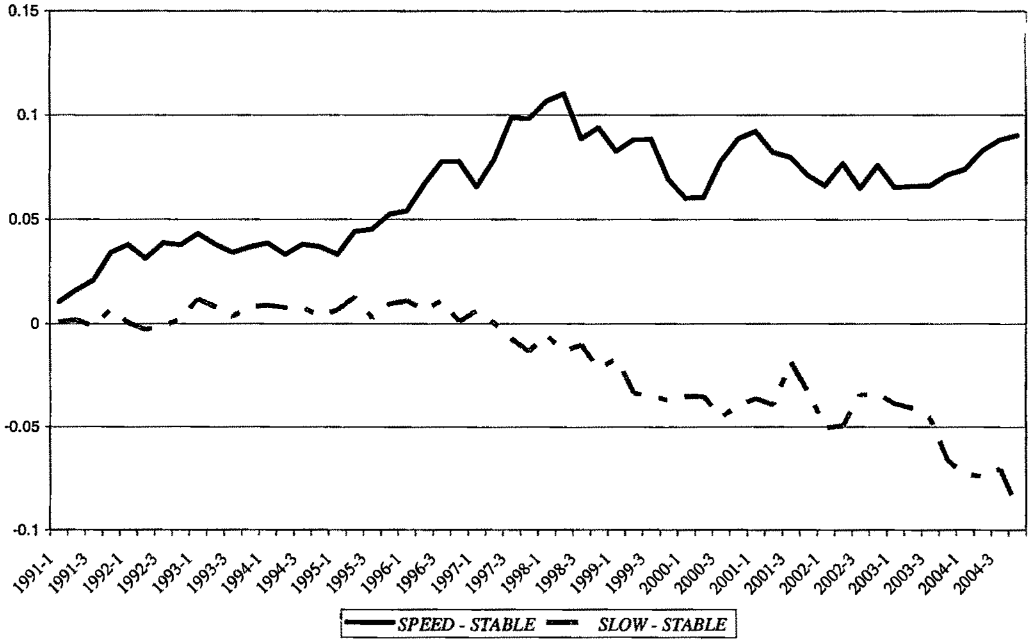
(*continued on next page*)

FIGURE 3 (continued)

Panel C: Mean Difference in *S&P500*



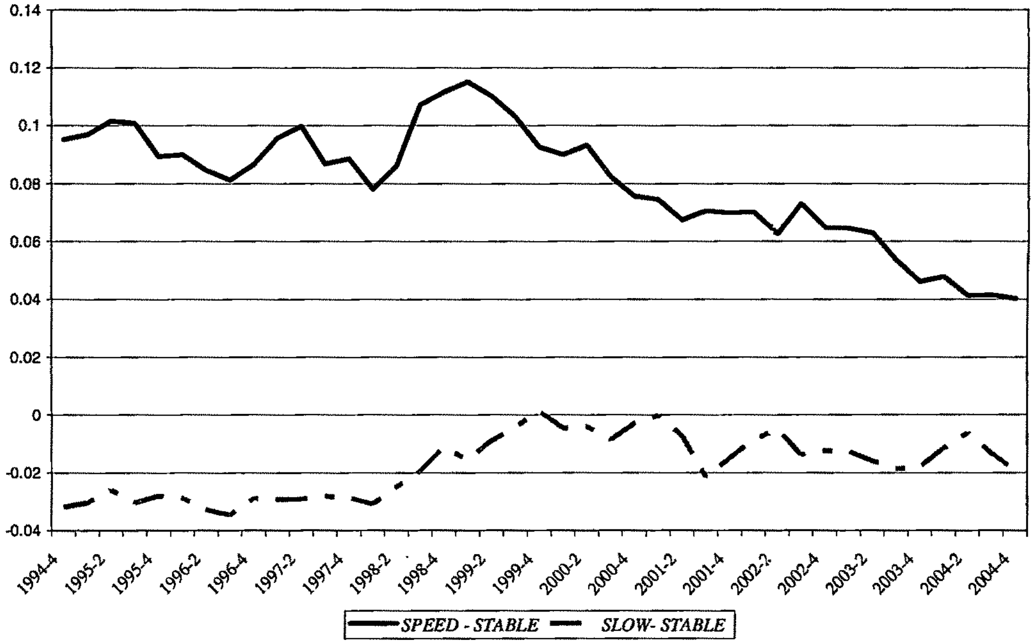
Panel D: Mean Difference in *CAP1B*



(*continued on next page*)

FIGURE 3 (continued)

Panel E: Mean Difference in *S&P1000*



Panel F: Mean Difference in *RATING*

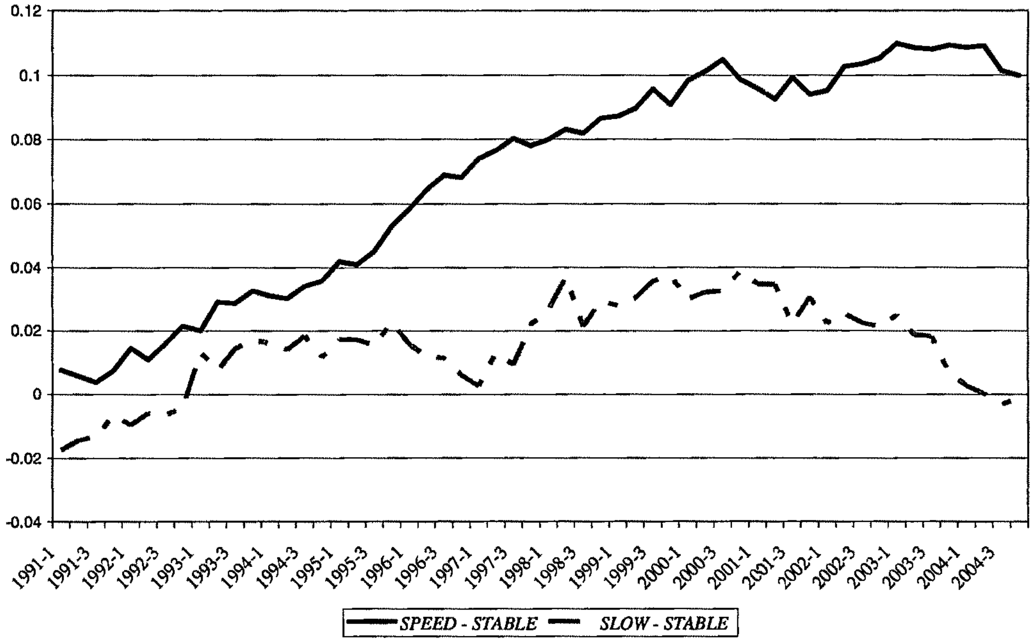


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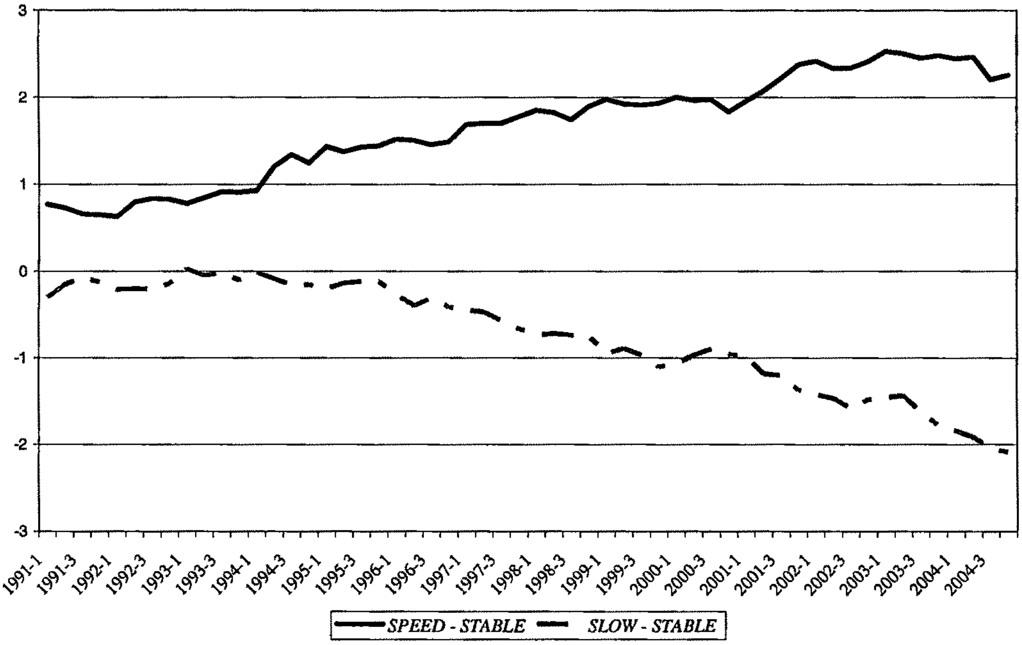
FIGURE 3 (continued)

Panel G: Mean Difference in Analyst Following



The above panels compare speeding versus slowing firms in terms of data collection speed (Panel A), overall institutional ownership (Panel B), proportion of firms in major market indices (Panels C–E), proportion of firms with S&P credit ratings (Panel F), and number of analyst following (Panel G). To identify firms whose collection lags have trended upward versus downward over time, we run the following regression for each firm:

$$BLOM\_DL_i = \alpha + \beta TIME_q + \varepsilon_i$$

where $BLOM\_DL_i$ is the Z-transformed cross-sectional rank (Blom 1958) of S&P's data collection lag (*DL*) by calendar year. The Z-scores are calculated annually to eliminate any inter-temporal effects of EDGAR on Compustat collection speed. $TIME_q$ is a time trend variable for calendar quarters over the period 1991Q2–2004Q4 (ranging from 1 to 55). We identify firms as having inter-temporally faster (slower) information dissemination speed if their β is significantly negative (positive) at the two-tailed level of 5 percent and assign them to the Group *SPEED* (*SLOW*). All other firms are defined as stable firms (denoted as *STABLE*). We use the stable firms as a benchmark and graph mean values of *SPEED* and SLOW groups as deviations from those of the *STABLE* group.

(*INFOSTATUS*) to identify these cases. We then define the collection lag variable *DL_KNOWN* as equal to the actual collection lag *DL* (0) when *INFOSTATUS* equals 0 (1). Based on H4, we predict a negative slope coefficient for *DL_KNOWN* in the models with changes in transient and quasi-indexer ownership as the dependent variable. With respect to *INFOSTATUS*, while we do not make specific predictions, the slope would have the same sign to the extent *INFOSTATUS* proxies for the coarseness of the information environment. Following Ke and Petroni (2004), we include various control variables as well as fixed effects for firms and for time periods.

Table 5 reports the results of estimating model (2), with standard errors corrected for heteroscedasticity and clustering by both firm and time. Consistent with H4, the coefficient

TABLE 5
Firm and Time Fixed-Effects Regression of Changes in Institutional Ownership by Trading Type on S&P Data Collection Lag and Control Variables
(n = 223,894 firm-quarters)

This table reports the results of estimating the following model using OLS:

$$\Delta OWN_{i,q} = \beta_0 + \beta_1 DL\_KNOWN_{i,q} + \beta_2 INFOSTATUS_{i,q} + \delta_3 OWN_{i,q-1} + \delta_4 PORTWT_{i,q-1} + \delta_5 Ln(MVE_{i,q-1}) + \delta_6 BTM_{i,q-1} + \delta_7 RETQ24_{i,q} + \delta_8 RETQ1_{i,q} + \delta_9 RETQ0_{i,q} + \delta_{10} UE_{i,q} + \varepsilon_{i,q}.$$

	Pred. Sign	$\Delta TRA_{i,q}$	$\Delta QIX_{i,q}$	$\Delta DED_{i,q}$
$DL\_KNOWN_{i,q}$	−/−/?	−0.0162 (0.000)***	−0.0176 (0.000)***	0.0139 (0.000)***
$INFOSTATUS_{i,q}$	−/−/?	−0.2590 (0.213)	−0.5737 (0.005)***	0.3323 (0.001)***
$OWN_{i,q-1}$		−0.2615 (0.000)***	−0.2237 (0.000)***	−0.2204 (0.000)***
$PORTWT_{i,q-1}$		−0.8282 (0.001)***	−1.8016 (0.000)***	−0.0563 (0.035)**
$Ln(MVE)_{i,q-1}$		0.6695 (0.000)***	1.0937 (0.000)***	−0.0454 (0.392)
$BTM_{i,q-1}$		−0.1726 (0.085)*	0.4061 (0.006)***	−0.2951 (0.000)***
$RETQ24_{i,q}$		0.0025 (0.023)**	−0.0001 (0.913)	0.0005 (0.231)
$RETQ1_{i,q}$		0.0134 (0.000)***	−0.0009 (0.614)	0.0007 (0.237)
$RETQ0_{i,q}$		0.0086 (0.000)***	−0.0003 (0.776)	0.0000 (0.999)
$UE_{i,q}$		0.6853 (0.000)***	0.9926 (0.000)***	−0.1163 (0.350)
Adjusted R^2		0.1376	0.1063	0.1133

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*, **, *** Represent statistical significance at two-tailed 0.1, 0.05, 0.01 levels, respectively.

Two-tailed p-values are included in parentheses. The p-values are based on two-way clustered standard errors that adjust for correlations within firms and within time periods.

Following Ke and Petroni (2004), we include various control variables as well as fixed effects for firms and for time periods.

All variables are as defined in Appendix.

of *DL_KNOWN* is significantly negative in the ΔTRA and ΔQIX models. For an increase in dissemination lag equal to its inter-quartile range (15 weekdays), the transient and quasi-indexer ownerships decline by 0.24 (−0.0162 × 15) and 0.26 (−0.0176 × 15) percentage points, respectively. While we do not make specific predictions for dedicated institutions, we find that dedicated investors in fact increase their ownership interest in response to slowing information dissemination, again consistent with the arguments in Bushee and Noe (2000). Interestingly, in all three regressions, the coefficient of *INFOSTATUS* has the same sign as that of the collection lag variable, but is not statistically significant for transient investors.[20]

The slope estimates for the control variables are largely consistent with prior studies. The statistically significant negative coefficients for OWN_{iq-1} and $PORTWT_{iq-1}$ indicate that transient investors and quasi-indexers periodically monitor their positions to maintain a well-diversified portfolio. Dedicated institutions are less sensitive to past portfolio weight in a specific firm ($PORTWT_{i,q-1}$) due to their relational investing style that relies less on diversification. The strong positive effect of firm size ($Ln(MVE)_{i,q-1}$) on institutional ownership is consistent with transient investors (quasi-indexers) being drawn to stocks with a finer information environment leading to lower price impact of trades (a quick access to publicly available information). Not surprisingly, only transient investors react to short-run price changes (*RETQ*) and rebalance their portfolio. Similarly, only dedicated institutions show no sensitivity to short-term earnings information ($UE_{i,q}$).[21]

VI. ADDITIONAL SENSITIVITY ANALYSES

Alternative Measures for Institutional Ownership

We substituted the natural logarithm of the number of investors of each type that hold a stock at the end of each quarter for *INST*, *TRA*, *QIX*, and *DED*, and report the results in Table 6. For the Table 3 analysis, we still find that quasi-indexers play the dominant role in influencing Compustat collection speed. The significantly positive coefficient on *DED* is consistent with S&P shunning stocks held by dedicated investors. For the Table 5 analysis, our results remain unchanged using this alternative measure. Results (untabulated) are similar when we consider only the number of institutions with greater than 5 percent stock holdings.

Excluding Bank Holdings

Institutions classified as banks in the 13-f database hold, on average, 4.2 percent of the shares outstanding of our sample firms. A large majority (more than 75 percent) of these holdings are concentrated in banks classified as quasi-indexers. Given that banks face the strictest fiduciary standards for purposes of investment (Bushee 2001), we reran all analyses excluding bank ownership to test the robustness of our results. In Panel A of Table 7, we report the results of our Table 3 analysis and find that the holdings of all three categories of non-bank institutions are negatively associated with dissemination lag, but the significance for transient and dedicated investors is weak (two-tailed p-value = 0.161 and 0.081, respectively). In addition, the magnitude of the slope coefficient for quasi-indexers is at least eight times as large as each of the other two coefficients, and the differences are statistically significant (untabulated). Therefore, our conclusion that demand for dissemination speed is largely driven by the needs of quasi-indexers still holds for non-bank institutions. Panel B of Table 7 shows that our Table 5 results remain unaltered when we exclude bank holdings from the calculation of institutional ownership.

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[20] Given that *INFOSTATUS* is more likely to take the value of 1 during the fourth fiscal quarter than during the interim quarters, we modify model (2) and estimate separate slope coefficients for *INFOSTATUS* for interim and fourth quarters. The estimates and the statistical significance for the two coefficients are very similar.

[21] As a robustness check, we include additional control variables from Bushee and Noe (2000, Table 4) that are not considered in model (2) (i.e., lagged values of trading volume, beta, idiosyncratic risk, and changes in the following variables: leverage, dividend yield, earnings yield, sales growth, credit rating, and shares outstanding). The tenor of our results is unaffected by the inclusion of the additional control variables. Similar in spirit to Bushee and Noe (2000), we also include lagged quintiles of *DL_KNOWN* and lagged *INFOSTATUS* and find that the former is insignificant in any model, while the latter is statistically significant with the correct sign in all models.

TABLE 6
Sensitivity Analyses Using the Number of Institutions as a Proxy for Information Demand

Panel A: Replication of Table 3 (n = 293,577 firm-quarters)

	Pred. Sign	Regression 1	Regression 2
$NUMINST_{i,q}$	–	−0.0120 (0.000)***	
$NUMTRA_{i,q}$	?		−0.0035 (0.263)
$NUMQIX_{i,q}$	–		−0.0316 (0.000)***
$NUMDED_{i,q}$	?		0.0461 (0.000)***

Panel B: Replication of Table 5 (n = 223,894 firm-quarters)

	Pred. Sign	$\Delta NUMTRA_{i,q}$	$\Delta NUMQIX_{i,q}$	$\Delta NUMDED_{i,q}$
$DL\_KNOWN_{i,q}$	−/−/?	−0.0022 (0.000)***	−0.0011 (0.000)***	0.0021 (0.000)***
$INFOSTATUS_{i,q}$	−/−/?	−0.0457 (0.007)***	−0.0312 (0.000)***	0.0495 (0.005)***

*, **, *** Represent statistical significance at two-tailed 0.1, 0.05, 0.01 levels, respectively.

Two-tailed p-values are included in parentheses. The p-values are based on two-way clustered standard errors that adjust for correlations within firms and within time periods.

This table reports the results of Table 3 and Table 5 regressions using the number of institutional investors by investor types as a proxy for demand for information. *NUMINST*, *NUMTRA*, *NUMQIX*, and *NUMDED* are natural logarithms of the number of all institutions, transient investors, quasi-indexers, and dedicated investors, respectively.

All other variables are as defined in Appendix.

Simultaneous Estimation

We test our demand determinants hypotheses (H1 through H3) and our consequences hypothesis (H4) through models (1) and (2) (Tables 3 and 5). To alleviate possible simultaneity concerns, we estimate (1) with institutional ownership broken down by type, and the three equations represented by (2), jointly using three-stage least-squares (3SLS).[22] Untabulated results are qualitatively similar to those reported earlier, and all major inferences hold. One difference is that the slope coefficient of *DED* in model (1) becomes significantly positive.

Fama-MacBeth Regressions and Poolability Tests

As an additional sensitivity check, we ran the regressions separately by calendar year-quarter, and computed each test statistic as the mean coefficient divided by its time-series standard error with Newey-West correction for four lags. Untabulated results indicate that all major inferences of Table 3 and Table 5 are robust to the use of the Fama and MacBeth (1973) approach except for two instances: in Table 3, the coefficient of *DED* becomes significantly negative and that of *ARBRISK* becomes insignificant.

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[22] We are not aware of any statistical package that can jointly estimate Poisson (1) and OLS (2) as a system. To circumvent this problem, we convert *DL* (the dependent variable in (1)) to its natural logarithmic value.

TABLE 7
Sensitivity Analyses Excluding Bank Holdings from Institutional Ownership

Panel A: Replication of Table 3 (n = 293,577 firm-quarters)

Selected Coefficients	Pred. Sign	Regression 1	Regression 2
$INST_{i,q}$	−	−0.0013 (0.000)***	
$TRA_{i,q}$	?		−0.0003 (0.161)
$QIX_{i,q}$	−		−0.0032 (0.000)***
$DED_{i,q}$	?		−0.0004 (0.081)*

Panel B: Replication of Table 5 (n = 223,894 firm-quarters)

Selected Coefficients	Pred. Sign	$\Delta TRA_{i,q}$	$\Delta QIX_{i,q}$	$\Delta DED_{i,q}$
$DL\_KNOWN_{i,q}$	−/−/?	−0.0159 (0.000)***	−0.0204 (0.000)***	0.0118 (0.000)***
$INFOSTATUS_{i,q}$	−/−/?	−0.2633 (0.155)	−0.5647 (0.004)***	0.2935 (0.003)***

*, **, *** Represent statistical significance at two-tailed 0.1, 0.05, and 0.01 levels, respectively.

Two-tailed p-values are included in parentheses. The p-values are based on two-way clustered standard errors that adjust for correlations within firms and within time periods.

This table reports the results of Table 3 and Table 5 regressions excluding the holdings of banks from the measures of institutional ownership.

All variables are as defined in Appendix.

In addition, we conducted three different types of poolability tests: (1) comparing pooled coefficient estimates with the average Fama-MacBeth coefficients; (2) comparing coefficient estimates between two subperiods (1991–1997 versus 1998–2004); and (3) testing whether the period-by-period coefficient estimates show a time trend. While the tenor of our inferences is unaltered by the poolability tests, the slope of $QIX_{i,q}$ in the Table 3 regression is smaller in magnitude during 1998–2004 compared to 1991–1997 (−0.0018 versus −0.0031) but statistically significant in both periods. The Table 5 results are robust to the various poolability tests.

Random versus Fixed Effects

We use fixed-effects regressions as they allow for correlation between the unobserved heterogeneity and the included variables (Wooldridge 2003, 473). While the fixed- and random-effects specifications provide very similar coefficient estimates in the Table 3 regressions, the Hausman test clearly rejects the firm-level, random-effects specification for Table 5.

10-K versus 10-Q Dissemination Lags

We reran our Table 3 and Table 5 regressions for 10-Ks and 10-Qs separately and find that the magnitude of the negative slope coefficient for quasi-indexers in Table 3 is larger for 10-Ks (−0.004 versus −0.002). Similarly, our results indicate that the institutional investors are more sensitive to the collection speed differences for 10-Ks versus 10-Qs, although the coefficients in both samples are statistically significant in our Table 5 analysis.

In summary, our hypotheses test results hold when we conduct a battery of sensitivity analyses. However, a few observations are in order. First, while we have addressed the correlated omitted-effects problem with an extensive set of controls, we cannot eliminate the possibility that dissemination speed is merely proxying for other firm characteristics that lead to changes in institutional ownership. Second, establishing causality in archival research is admittedly challenging. However, the differential predictions for the determinants of dissemination speed versus the institutional investor response provide added comfort to our results. Third, Compustat's collection lag is unlikely to capture the timing of data availability to *all* institutional investors. Nevertheless, we understand that Compustat has a large market share in the institutional investor market for aggregated corporate accounting information, and expect its collection lag to proxy for the general demand in this market.

VII. CONCLUSION

Despite the fact that technological advances now make it possible for investors to obtain almost immediate access to accounting information released by firms in their periodic filings, the multi-billion dollar market for the services of data aggregators has grown in recent years. EDGAR has had a dramatic impact on the efficiency of such data aggregators: we document a 50 percent decline in S&P's collection lag for periodic filings in the post-EDGAR period. Despite increased efficiencies over time, substantial cross-sectional variation persists in information dissemination speed across firms. Changing technologies (e.g., XBRL) will likely further improve the overall efficiency of data aggregators. However, unless supply costs are virtually eliminated, cross-sectional differences in information dissemination speed are likely to persist, much as they have in the wake of EDGAR.

In broad terms, we hypothesize that information dissemination policies of data aggregators are driven by differences in the value that market participants place on speedy access to comparable accounting information pertaining to different firms. Information dissemination policies, in turn, influence the information environment of firms, thereby affecting the trading decisions of the investor clientele.

Our findings are consistent with these predictions. We find that S&P disseminates accounting information faster for firms with higher quasi-indexer institutional ownership, lower transaction costs (proxied by price and trading volume), and lower idiosyncratic risk. Our results are consistent with arguments in past research that quasi-indexers rely on corporate accounting information as a low-cost performance-monitoring device, and that high arbitrage risk or transaction costs deter institutional investors from exploiting accounting-based mispricing. We also find that dissemination speed increases for stocks listed in major indices, possibly due to heightened investor awareness of index stocks.

Regarding consequences, we find evidence that both quasi-indexers and transient investors gravitate to stocks with faster information-dissemination speed. Past research has shown that transient investors are better able to exploit their informational advantages and implement their trading strategies in the presence of a richer information environment. Quasi-indexers rely on accounting information to monitor the performance of firms in their portfolio. Our results are consistent with these arguments.

Taken together, these results provide a first look at the role played by capital market data aggregators in the informational efficiency of the marketplace. We show that data aggregators respond to the needs of the marketplace by disseminating corporate accounting information in standardized, comparable form at differential speeds. Their response, in turn, helps shape the information environment of firms and affects investor trading behavior. Future research could explore other consequences of information dissemination by data aggregators, including effects on market efficiency.

APPENDIX
VARIABLE DEFINITIONS

Variables Used in Tables 2, 3, 4, 6, and 7

$DL_{iq,tp}$ = the number of weekdays from the 10-K/10-Q filing date t of firm i for fiscal quarter q to Compustat production date p (*FINALQPRD*) when the firm's finalized financial data first appeared in Compustat's Research Insight database;

$INST_{i,q}$ = percentage of institutional holdings of firm i (winsorized to 100) at the end of the calendar quarter at or immediately preceding the current fiscal quarter q;

$TRA_{i,q}$ = percentage of stock ownership of firm i (winsorized to 100) held by transient institutions at the end of the calendar quarter at or immediately preceding the current fiscal quarter q;

$QIX_{i,q}$ = percentage of stock ownership of firm i (winsorized to 100) held by quasi-indexers at the end of the calendar quarter at or immediately preceding the current fiscal quarter q;

$DED_{i,q}$ = percentage of stock ownership of firm i (winsorized to 100) held by dedicated institutions at the end of the calendar quarter at or immediately preceding the current fiscal quarter q;

$NUMINST_{i,q}$ = natural logarithm of number of institutions of firm i at the end of the calendar quarter at or immediately preceding the current fiscal quarter q;

$NUMTRA_{i,q}$ = natural logarithm of number of transient institutions of firm i at the end of the calendar quarter at or immediately preceding the current fiscal quarter q;

$NUMQIX_{i,q}$ = natural logarithm of number of quasi-indexers of firm i at the end of the calendar quarter at or immediately preceding the current fiscal quarter q;

$NUMDED_{i,q}$ = natural logarithm of number of dedicated institutions of firm i at the end of the calendar quarter at or immediately preceding the current fiscal quarter q;

$SPECTRUM_{i,q}$ = 1 if institutional ownership data of firm i at the end of the calendar quarter at or immediately preceding the current fiscal quarter q is available from Thomson Financial Spectrum database, and 0 otherwise;

$ARBRISK_{i,q}$ = standard deviation of residuals from a regression of firm-specific daily returns on the returns of the CRSP equally weighted market index over fiscal quarter q for firm i (a minimum of five observations is required); we multiply the standard deviation of residuals by 100 and winsorize it at its 99.5 percentile value;

$PRICE_{i,q}$ = closing stock price of firm i as of fiscal quarter end q, winsorized at its 99.5 percentile value;

$VOLUME_{i,q}$ = daily trading volume (in millions of dollars) of firm i averaged over fiscal quarter q, winsorized at its 99.5 percentile value;

$S\&P500_{i,q}$ = 1 if firm i is in the S&P 500 index at the end of fiscal quarter q, and 0 otherwise;

$CAP1B_{i,q}$ = 1 if $S\&P500_{i,q}$ equals 0 and the market capitalization of firm i is greater than $1 billion dollars at the end of fiscal quarter q, and 0 otherwise;

$S\&P1000_{i,q}$ = 1 if $CAP1B_{i,q}$ equals 0 and firm i is in the S&P MidCap 400 or SmallCap 600 index at the end of fiscal quarter q, and 0 otherwise;

$RATING_{i,q}$ = 1 if S&P credit rating is available for firm i in fiscal quarter q, and 0 otherwise;

$FINSERV_i$ = 1 if financial service firms (SIC 6000–6999), and 0 otherwise;

$UTILITY_i$ = 1 if utilities (SIC 4900–4999), and 0 otherwise;

$SPECIAL_{i,q}$ = 1 if a special item is reported in the Compustat database for firm i for fiscal quarter q, and 0 otherwise;

$EXTRAORD_{i,q}$ = 1 if an extraordinary item is reported in the Compustat database for firm i for fiscal quarter q, and 0 otherwise;

$CONCUR_{i,q}$ = 1 if the earnings announcement date of firm i matches its SEC periodic report filing date for fiscal quarter q, and 0 otherwise;

$\#EA_p$ = number of earnings announcements (in 000s) made by all companies followed by Compustat during the production cycle ending at date p;

$\#FL_p$ = number of 10-K/10-Q filings (in 000s) made by all companies followed by Compustat during the production cycle ending at date p;

$\#EA\_UPDATE_p$ = number of preliminary earnings announcements (in 000s) from which data was collected during the Compustat production cycle ending at date p;

$\#FL\_UPDATE_p$ = number of periodic SEC reports (in 000s) from which data was collected during the Compustat production cycle ending at date p;

EA_BKLG_{p-1} = number of uncollected earnings announcements (in 000s) at the end of Compustat production cycle $p-1$;

FL_BKLG_{p-1} = number of uncollected 10-K/10-Q filings (in 000s) at the end of Compustat production cycle $p-1$;

$QTREND_p$ = 1 if the end of production cycle p is the last business day in a quarter, and 0 otherwise; and

$YREND_p$ = 1 if the end of production cycle p is the last business day in a year, and 0 otherwise.

Variables Used in Tables 5, 6, and 7

Note: Variables measured for a fiscal quarter are based on information from the most recent fiscal quarter prior to the calendar quarter q.

$\Delta TRA_{i,q}$ = change in percentage of stock ownership of firm i held by transient institutions over the calendar quarter q;

$\Delta QIX_{i,q}$ = change in percentage of stock ownership of firm i held by quasi-indexers over the calendar quarter q;

$\Delta DED_{i,q}$ = change in percentage of stock ownership of firm i held by dedicated institutions over the calendar quarter q;

$INFOSTATUS_{i,q}$ = 1 if by the end of the institutional ownership measurement date the firm *i* has not yet filed its periodic SEC report or Standard & Poor's has not yet collected the information from the periodic report for the fiscal quarter, and 0 otherwise;

$DL\_KNOWN_{i,q}$ = $DL_{i,q}$ (0) when $INFOSTATUS_{i,q-1}$ equals 0 (1);

$DL_{i,q}$ = the number of weekdays from the 10-K/10-Q filing date *t* of firm *i* for the fiscal quarter to Compustat production date when the firm's finalized financial data first appeared in Compustat's Research Insight database;

$OWN_{i,q-1}$ = $TRA_{i,q-1}$, $QIX_{i,q-1}$, or $DED_{i,q-1}$, when the dependent variable is $\Delta TRA_{i,q}$, $\Delta QIX_{i,q}$, or $\Delta DED_{i,q}$, respectively;

$PORTWT_{i,q-1}$ = mean portfolio weight of firm *i*'s stock held by the institutions included in the calculation of the dependent variable (weighted by the total market capitalization of all stocks held by the same institutions), calculated as of the end of the calendar quarter $q-1$;

$Ln(MVE)_{i,q-1}$ = logarithm of market value of common equity of firm *i* at the beginning of fiscal quarter *q*;

$BTM_{i,q-1}$ = ratio of book value of equity to market value of equity of firm *i* at the beginning of fiscal quarter *q*;

$RETQ24_{i,q}$ = buy-and-hold raw return in percentage of firm *i* for two to four calendar quarters prior calendar quarter *q*;

$RETQ1_{i,q}$ = buy-and-hold raw return in percentage of firm *i* for the calendar quarter *q*;

$RETQ0_{i,q}$ = buy-and-hold raw return in percentage of firm *i* from 30 to 3 trading days before the earnings announcement date of fiscal quarter *q*; and

$UE_{i,q}$ = change in earnings before extraordinary items of firm *i* from fiscal quarter $q-4$ to *q*.

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Does Public Ownership of Equity Improve Earnings Quality?

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ABSTRACT: We compare the quality of accounting numbers produced by two types of public firms—those with publicly traded equity and those with privately held equity that are nonetheless considered public by virtue of having publicly traded debt. We develop and test two hypotheses. The "demand" hypothesis holds that earnings of public equity firms are of higher quality than earnings of private equity firms due to stronger demand by shareholders and creditors for quality reporting. In contrast, the "opportunistic behavior" hypothesis posits that public equity firms, because their managers have a greater incentive to manage earnings, have lower earnings quality than their private equity peers. The results indicate that, consistent with the "opportunistic behavior" hypothesis, private equity firms have higher quality accruals and a lower propensity to manage income than public equity firms. We further find that public equity firms report more conservatively, in line with their greater litigation risk and agency costs.

Keywords: *accruals; conservatism; earnings management; earnings quality; private and public firms.*

Data Availability: *Data are available from sources identified in the study.*

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I. INTRODUCTION

The quality of accounting information is influenced by an array of factors, most of which stem from the demand for such information for use in contractual arrangements and from the incentives and opportunities of management to manage the reported numbers. Both the demand for quality accounting information for contractual purposes and management incentives to adjust the reported earnings are likely to be influenced by whether the equity of the company is privately held or publicly traded. In this study, we examine the differential earnings quality of private equity and public equity firms in order to shed light on how public ownership of equity affects the quality of firms' earnings. Because earnings "quality" has multiple dimensions, in our tests we examine a number of attributes that have been associated by previous research with the notion of earnings quality.

The influence that public or private ownership has on the quality of accounting numbers has been examined in limited contexts by past studies. Beatty et al. (2002), Burgstahler et al. (2006), and Penno and Simon (1986) focus on the difference between public and private firms with respect to one dimension of earnings quality—the extent to which earnings are managed. The association between ownership type and another earnings attribute, conservatism, is examined by Ball and Shivakumar (2005). Because financial data of privately owned firms is generally unavailable, these studies are restricted to regulated industries such as banking and insurance for which financial reports of both public and private companies are filed with industry regulators (e.g., Beatty et al. 2002) or to countries such as the U.K. in which accounting information is available for private companies because they must publicly file financial statements (e.g., Ball and Shivakumar 2005).

The results of these studies on the differential earnings quality of public versus private companies are conflicting. Beatty et al. (2002) find that public firms have a greater propensity to manage earnings than private firms, whereas Burgstahler et al. (2006) report the opposite. Further, to the extent that conservatism is viewed as an earnings quality trait, this finding by Beatty et al. (2002) is ostensibly in contrast to the finding by Ball and Shivakumar (2005), who use the extent of reporting conservatism to assess earnings quality.

The results of these studies, while insightful, cannot easily be generalized. The examination of firms in a regulated industry provides results for a single industry with unique financial reporting issues. Further, the extent to which the results based on samples consisting of European companies generalize to U.S. firms is not clear due to differences in these countries' reporting regimes.

Our study extends the literature on the effect of ownership type on earnings quality by examining a broader sample of U.S. firms (in non-regulated industries) and by considering several measures of earnings quality. Specifically, we compare the quality of accounting numbers between private and public equity firms along three dimensions: persistence of accruals, estimation error in the accrual process, and prevalence of earnings management. We also compare the degree of conservatism between these two groups of firms.

Our sample of U.S. companies consists of two types of public companies: those with publicly traded equity (hereafter, public equity firms) and those with privately held equity that are considered public companies because they have publicly traded debt (hereafter, private equity firms). Both types of firms are subject to identical SEC reporting and disclosure requirements. Hence, our tests control for many of the factors affecting the comparison of earnings quality across countries, such as legal institutions, tax laws, securities regulations and the extent of their enforcement, as well as reporting and disclosure requirements. We are thus able to identify more precisely how the ownership structure of the company affects its earnings quality. Relying on this unique sample of U.S. companies and on a broader set of earnings quality attributes, our study sheds light on the question of the effect of ownership type on earnings quality. Further, it resolves the question of whether the apparent conflicting results of past studies are due to differences in the examined samples or an inherent negative association between the different measures employed to assess earnings quality.

Findings indicate that the accounting numbers produced by public equity and private equity firms exhibit different reporting attributes. While public equity firms have a lower quality of accruals in terms of their persistence and estimation error and further exhibit a greater propensity to manage earnings, they also report more conservatively (in terms of timely loss recognition) than their private firm counterparts.

This study is the first to analyze the quality of accounting information generated by firms whose debt, but not equity, is publicly traded, comparing such firms to public equity firms. We further examine the different incentives and opportunities that management of these two types of firms has to affect the reported numbers. By extending the literature on earnings quality and the differential quality between public versus private companies, this study enhances our understanding of how, and the extent to which, management incentives and investor demand for earnings quality impact financial reporting.

The study contributes to the existing research in two main respects. First, we consider a spectrum of attributes related to the concept of earnings quality rather than a single attribute of earnings quality such as earnings management. Second, by examining a unique sample of privately held public companies, the study highlights how the presence of public equity investors affects management's reporting behavior, controlling for the regulatory environment as well as the disclosure and reporting regimes.

In the next section, the characteristics of the sample firms are described. The hypotheses are developed in Section III, followed by a discussion of the various measures used to assess earnings quality. The sample and data are described in Section V. The results are provided in Section VI. Concluding remarks are provided in the last section.

II. PRIVATE EQUITY FIRMS WITH PUBLIC DEBT

Under Sections 13 and 15(d) of the Securities Exchange Act of 1934, firms with publicly traded equity as well as those with privately held equity and publicly traded debt are subject to identical financial reporting and disclosure regulations. While both are considered to be public firms, private equity firms differ from public equity firms in various ways because their shares are not publicly traded. For example, because of the lack of publicly traded stock, takeovers of private equity firms cannot be accomplished through open market stock purchases and their equity-based compensation is not tied to market stock prices.

There is a large body of theoretical literature examining the motivations for becoming a public company. These motivations include increased liquidity (Amihud and Mendelson 1988), having greater access to capital (Welch 1989), optimizing the exit opportunity for current shareholders (Zingales 1995; Mello and Parsons 1998; Stoughton and Zechner 1998; Black and Gilson 1998), diversifying the founding shareholders' wealth (Leland and Pyle 1977) and improving the owners' ability to monitor the firms' managers (Jensen and Meckling 1976; Pagano and Röell 1998; Holmström and Tirole 1993; Bolton and von Thadden 1998). Implicit or explicit in this literature is the assumption that becoming public means having public ownership. However, a private firm could also become public by issuing public debt. Some of the benefits of public ownership, such as creating an easier exit for the owners or generating greater liquidity for current shareholders, do not exist for private firms that issue public debt, while others, such as improving owners' monitoring capability, might. Indeed, there has been little attention paid by the literature to the various factors that influence a firm's choice "to go public" by issuing public equity or public debt. Helwege and Packer (2003) show that among public debt firms, those that have private equity are younger, more leveraged, and appear to have fewer growth opportunities than public equity firms. Similar findings are reported by Katz (2009), who compares firm characteristics of privately held firms before and after their equity IPO. We confirm these results in this study. As reported later, our sample of companies with public debt that are privately owned are more leveraged, have lower growth rates, and are less profitable than firms with public debt that have publicly held equity.

The factors that lead private equity owners to issue public debt instead of, or prior to, issuing public equity, while having a bearing on the "pecking order" theory, have not been directly researched. However, research on the priority of financing sources suggests that issuing public debt involves more costly disclosure of propriety information than does issuing private debt (Campbell 1979; Myers 1984; Yosha 1995). All else being equal, one would thus expect firms to rely more on private debt when the public disclosure of firm-specific information is costlier (Dhaliwal et al. 2008). Credit quality also appears to play a role in firms' debt-financing decisions. Empirical evidence indicates that firms with higher credit ratings tend to borrow from public sources, those with intermediate ratings borrow from banks, and firms with the lowest credit quality borrow from private lenders (Cantillo and Wright 2000; Denis and Mihov 2003; Bharath et al. 2008). These findings suggest that firms that issue public debt are likely to be financially stronger than firms that do not have publicly held debt, an expectation confirmed in our sample.[1]

The above discussion suggests that the decision about which type of public security to issue, debt or equity, is endogenous. Therefore, as explained below, our tests on the association between financial reporting attributes and ownership type control for firm characteristics related to this choice.

III. HYPOTHESES ON EARNINGS QUALITY AND OWNERSHIP STRUCTURE

A natural starting point for developing hypotheses about the quality of financial reporting of private equity firms with publicly traded debt is to consider those posed by previous research on the differential quality of financial reports for public versus private firms. Interestingly, these hypotheses lead to conflicting predictions. On one hand, the demand for high-quality reporting by public equity firms is hypothesized to be stronger since accounting information is the main type of information contractually available to public equity holders. In addition, public equity firms have stronger incentives to improve their accounting and disclosure policies and enhance their financial transparency so as to mitigate potential lawsuits (consistent with the findings of Skinner 1997) and to reduce the cost of their equity capital.

Based on these considerations, one might expect the demand for high-quality accounting information to be greater for public equity firms than for private equity firms. This "demand" hypothesis is advanced by Ball and Shivakumar (2005) and, by implication, to studies on the differential demand for financial reporting quality between countries that resolve information asymmetries via "insider access" as compared with countries that alleviate such asymmetries through "arm's length" public disclosures.

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[1] Holders of public debt are protected by the Trust Indenture Act of 1939 which requires that firms receive the unanimous consent of the public debt holders to modify the terms of the bond indenture agreements. Given the dispersed holdings of most bond issues, this unanimity requirement is likely to make it more difficult for public debt holders to renegotiate debt contracts and to effectively monitor the borrowing firm's performance as compared to private lenders. (Smith and Warner [1979] discuss standard contractual arrangements for public and private debt.)

On the other hand, management of public firms is under continuous pressure by investors to meet certain performance benchmarks. For example, management has incentives to manage earnings to meet analysts' forecasts (e.g., Degeorge et al. 1999; Bartov et al. 2002) or to avoid reporting losses (e.g., Hayn 1995; Burgstahler and Dichev 1997) or earnings decreases (e.g., Burgstahler and Dichev 1997; Barth et al. 1999). Further, management may have a personal stake in the firm's stock price as a result of having stock-based compensation or stock ownership. In contrast, managers of private firms are not exposed to stock market repercussions arising from financial disclosures, nor are they as likely to be as influenced by incentives arising from stock-based compensation.[2] In addition, because private firms are more closely held, they tend to have fewer shareholders. Those owners are likely to be more closely involved in the management of the firm and thus face a lower cost of acquiring information, thereby reducing management's incentive to manage earnings. This "opportunistic behavior" hypothesis, which posits that managers of public equity firms have a greater propensity to manage earnings than managers of private equity firms because of stock price considerations, is supported by the findings of Beatty et al. (2002) and the survey results of Penno and Simon (1986).

Arguably, earnings management could be present among private equity firms. Managers of private firms may have incentives to manage earnings due to the presence of earnings-based bonuses as well as to avoid violating earnings-based debt covenants. Further, the latitude to manage earnings may be present, since private equity firms are likely to be less exposed than public equity firms to litigation risk and cost of equity capital considerations. The extent to which these two types of firms engage in earnings management is an empirical question. However, because of the strength of the *a priori* arguments in its favor, we formalize the "opportunistic behavior" hypothesis as a one-sided hypothesis, namely, that managers in public equity firms have a stronger incentive to manage earnings than their private equity counterparts. Note that the "demand" and "opportunistic behavior" hypotheses are not mutually exclusive. The observed differential in financial reporting quality likely reflects the net effect of both influences.

Even though we do not examine "purely" private firms, that is, firms that are privately owned and have no public debt, these firms are hypothesized to have both a weaker demand for accounting quality and weaker incentives to manage income than privately owned firms with public debt. Public debt holders of privately owned firms are likely to demand high accounting quality since the financial statements are their primary source of information about the firm. Unlike private lenders, public debt holders are not privy to inside information before extending credit, nor are they entitled to receive information on the extent of compliance with the terms of the debt contract beyond that contained in the prospectus and in subsequent SEC-mandated public reports and disclosures.[3]

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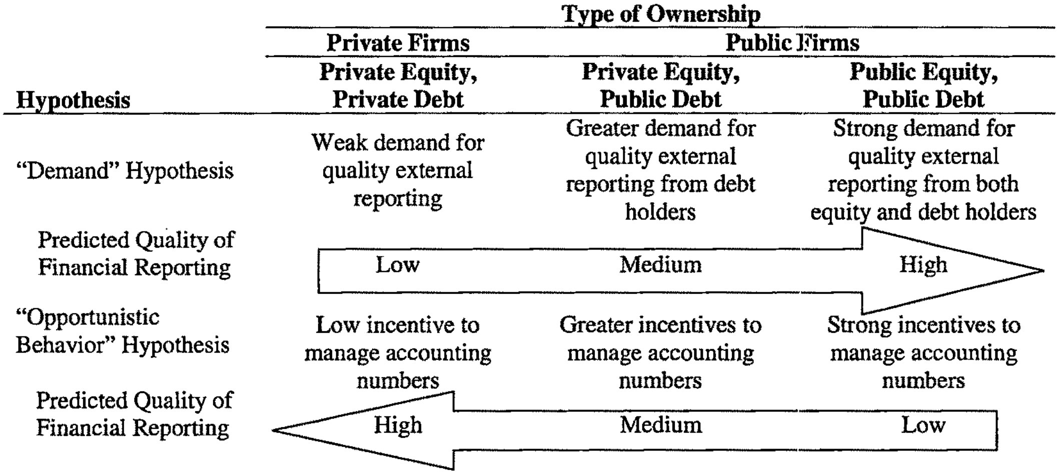
[2] To gain insight into the compensation arrangements of private equity firms, we examined a sample of private firms with public debt that subsequently issued public equity. For each of these firms, we compared the CEO's compensation package prior to the IPO (when the firm was private but had public debt) and subsequent to the IPO. The findings indicate that earnings-based compensation is as frequent in the pre-IPO period as it is in the post-IPO period. Further, the percentage of CEOs receiving stock options as part of their compensation package does not increase significantly after the IPO although, as might be expected, the monetary value of the options is significantly larger after the IPO. Furthermore, since private equity firms lack publicly traded stock, the value of their shares is determined, if needed, through appraisals. This evidence is indicative of the differences in the compensation structure of public and private equity firms.

Because they have less access to private information, less effective ways of monitoring and disciplining management, and less efficient tools for liquidation or renegotiation in the event of financial distress than private lenders, public debt holders are likely to have a greater demand for high-quality accounting numbers. Further, since public debt holders have less access to private information than private lenders, firms with public debt, whether publicly or privately owned, have stronger incentives to manage earnings than firms with no debt or with only private debt. Still, this "opportunistic behavior" is hypothesized to be less pronounced among private equity firms since capital market considerations are not a concern for these firms except, perhaps, in situations in which a firm is on the brink of violating its debt covenants or making a public equity offering.

Since the demand for accounting quality as well as the opportunistic behavior of managers of public equity firms may be affected by whether they have public debt, we control for the presence of public debt by comparing the accounting quality of private equity firms with public debt to the accounting quality of public equity firms with public debt.

Figure 1 summarizes the predictions of the two hypotheses with respect to the accounting quality of firms with different types of stakeholders. For completeness, we also include predictions for the group of entirely private firms (those with private equity and private debt) for which no information is publicly available in the U.S.

FIGURE 1
Type of Ownership and Expected Quality of Financial Reporting



	Type of Ownership		
	Private Firms	**Public Firms**	
Hypothesis	**Private Equity, Private Debt**	**Private Equity, Public Debt**	**Public Equity, Public Debt**
"Demand" Hypothesis	Weak demand for quality external reporting	Greater demand for quality external reporting from debt holders	Strong demand for quality external reporting from both equity and debt holders
Predicted Quality of Financial Reporting	Low	Medium	High
"Opportunistic Behavior" Hypothesis	Low incentive to manage accounting numbers	Greater incentives to manage accounting numbers	Strong incentives to manage accounting numbers
Predicted Quality of Financial Reporting	High	Medium	Low

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[3] Theoretical and empirical evidence suggest that access to information and the strength of the monitoring mechanism is likely to differ between public and private debt. For example, some studies hypothesize that private debt financing has an advantage over public debt in terms of monitoring efficiency (e.g., Diamond 1984; Boyd and Prescott 1986; Berlin and Loyes 1988), access to private information (Fama 1985), and the efficiency of liquidation and renegotiation in financial distress (Chemmanur and Fulghieri 1994; Gertner and Scharfstein 1991). The empirical evidence is generally consistent with these expectations (e.g., Kwan and Carleton 2004). See Johnson (1997) for a summary of these findings.

An additional characteristic that could affect earnings management and that may differ between public and private firms is the degree of ownership concentration. High ownership concentration, while creating the ability and incentives for managers to manage reported earnings, leads at the same time to a closer and more effective scrutiny of management by major shareholders. As a result, the net effect on the extent of earnings management is not obvious. The evidence, however, is more consistent with the notion of a positive association of ownership concentration and earnings management (e.g., Leuz et al. 2003; Haw et al. 2004). Although not controlling for ownership concentration works against rejecting our null hypothesis of greater earnings management among publicly owned firms, to explore this issue further we conduct tests on a subsample of firms with sufficient data to control for the level of ownership concentration.

IV. MEASURES OF ACCOUNTING QUALITY

The concept of earnings quality is elusive. The literature on "earnings quality" does not provide a clear definition of that "quality." It does, however, identify different attributes that are associated with or reflective of earnings quality.

Penman and Zhang (2002, 237), while recognizing the lack of consensus on the definition of earnings quality, define the term to mean that "reported earnings ... is a good indicator of future earnings." They consider high-quality earnings to be "sustainable earnings" and, correspondingly, deem an accounting system that produces unsustainable earnings as being of poor quality. They show that, in addition to the disruptive effect on earnings sustainability caused by changes in accounting methods and estimates, hidden reserves (such as those created by the use of LIFO or expensing of R&D) reduce the sustainability of earnings by providing more opportunity for earnings management. Richardson et al. (2005) and implicitly Sloan (1996) suggest a related dimension of earnings quality—the reliability of accruals as captured by earnings persistence. Richardson et al. (2005, 438) hypothesize and find that "less reliable accruals result in lower earnings persistence." Another related measure of earnings quality, the conformity with GAAP (as captured by SEC enforcement actions), is employed by Dechow et al. (1996) and Bradshaw et al. (2001).

Dechow and Dichev (2002) suggest another aspect of earnings quality—the strength of the relation between current accruals and past, present, and future cash flows. Accordingly, they propose a model for expected accruals and interpret the deviation from this "expected" value as the estimation error in accruals, which they use as a measure of earnings quality. This measure is affected by firm characteristics such as the length of the business cycle as well as by earnings management. Ball and Shivakumar (2005, 84) define reporting quality in general terms as "the usefulness of financial statements to investors, creditors, managers, and all other parties contracting with the firm." They view accounting conservatism in the form of asymmetric timeliness in recognizing losses versus gains as a dimension of earnings quality. However, as discussed later, equating conservative reporting with earnings quality is not universally accepted.

In summary, no single measure of accounting numbers captures all of the dimensions of earnings quality. Previous studies have identified a number of attributes associated with different aspects of earnings quality such as accrual persistence, estimation errors in the accrual process, and the absence of earnings management. These quality traits as well as the conservatism attribute are discussed in the following paragraphs.

Accrual Persistence

Our first measure of earnings quality is based on the differential persistence of accruals relative to cash flows. We measure persistence using the following regression:

$$OI_{i,t+1} = \alpha + \beta_1 CF_{i,t} + \beta_2 ACCR_{i,t} + \varepsilon_{i,t} \tag{1}$$

where *OI* is operating income after depreciation, *CF* is the operating cash flow component of earnings defined as *OI* minus *ACCR*, and *ACCR* is the accrual component of earnings measured as the change in net operating assets (NOA) from year $t-1$ to t.[4] In Regression (1) and throughout, the subscripts *i* and *t* refer to the firm and year, respectively. All variables in Regression (1) are standardized by NOA_{t-1}. The incremental contribution of accruals is determined by the magnitude and significance of β_2.

We account for the possible endogeneity in the decision to issue public equity by using the Heckman (1979) two-stage procedure, following the approach used by Ball and Shivakumar (2005) and Katz (2009). In the first stage, size (measured alternatively as total assets and sales), growth in sales, leverage (defined as total debt divided by total assets), profitability (defined as operating income divided by net operating assets, RNOA), the quick ratio (defined as current assets excluding inventory and prepaid expenses divided by current liabilities), age (years since incorporation), auditor quality (an indicator variable assigned the value 1 (0) if the firm is (is not) audited by one of the large national firms), and the length of the operating cycle (the sum of the average collection period and days in inventory) serve as predictors of the equity choice in a PROBIT model.[5] Estimates of the PROBIT model are used to compute the inverse Mills ratio for each sample firm. In the second stage, we include the inverse Mills ratio as a control variable in Regression (1), allowing the coefficient to vary between the two groups of firms.[6]

Estimation Error in the Accruals Process

Accruals provide information about future cash flows. To the extent that the accruals process is free of estimation errors, accruals and earnings will be more representative of future cash flows. Building on this notion, the second attribute of earnings quality that we consider is the degree of stability in the relation between cash flows and accruals. This measure as proposed by Dechow and Dichev (2002) and modified by McNichols (2002) and Francis et al. (2005), is based on the variance of the residuals from the following model:

$$TCA_{i,t} = \beta_0 + \beta_1 CFO_{i,t-1} + \beta_2 CFO_{i,t} + \beta_3 CFO_{i,t+1} + \beta_4 \Delta Rev_{i,t} + \beta_5 PPE_{i,t} + \varepsilon_{i,t} \tag{2}$$

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[4] Net operating assets equal the book value of common and preferred equity, plus total debt, minus the sum of cash, short-term investments and investment and advances, plus minority interest.

[5] Extending this test, we included proxies for expected growth (average of the realized annual growth in sales in the subsequent three years), proximity to covenant violation (net income before interest divided by the standard deviation over ten years of net income before interest), and ownership concentration (an indicator variable assigned the value of 1 (0) if the firm has (does not have) concentrated ownership (defined as private equity firms, which presumably have high ownership concentration, and public equity firms in the top quartile of the distribution of the percentage of holdings by the top five institutional holders). The results of this extension (which due to data requirements is based on considerably smaller samples) are similar to those tabulated.

[6] We conducted all analyses on a subgroup of 223 private equity firms that once were, or later became, public equity firms. Each private equity firm thus serves as its own control, further mitigating endogeneity concerns. All results (untabulated) remain qualitatively similar.

where *TCA* is total current accruals,[7] *CFO* is cash flows from operations (measured as income from continuing operations less total accruals, where total accruals equal total current accruals minus the depreciation and amortization expense), ΔRev is the change in revenues from year $t-1$ to t, and *PPE* is the balance of property, plant, and equipment (on a gross basis). All variables in Regression (2) are scaled by average total assets in year t. In line with Francis et al. (2005), we estimate Regression (2) cross-sectionally for each industry (defined by two-digit SIC codes) with at least 20 firm observations in a given year.

Our second quality measure is the variability (assessed by the standard deviation) of the residuals from Regression (2). The rationale underlying use of this measure is that the higher is the variability of the relation between earnings and cash flows, the lower is the quality of the accruals and, since earnings incorporate accruals, the lower is the quality of earnings.

Because the standard deviation of accruals may reflect the volatility of the firm's operations rather than reporting quality per se (see Liu and Wysocki 2006), we follow the suggestion in Verdi (2006) an consider an additional *relative* measure of accruals quality, defined as the ratio of the standard deviation of the residuals from Regression (2) to the standard deviation of total current accruals.

Absence of Earnings Management

It is difficult to determine the presence or absence of earnings management, since the series of unmanaged earnings is not observable. However, certain patterns in earnings are indicative of the presence of earnings management. One such pattern is the concentration of earnings numbers just above some earnings threshold (Degeorge et al. 1999). For example, earnings clustered just above zero have been interpreted as reflecting earnings management to avoid reporting a loss. Earnings growth in the current quarter relative to the same quarter the previous year of zero or a slightly positive amount may suggest that the current period's earnings have been managed to avoid reporting an earnings decline. Similarly, earnings that result in no, or just a small, positive earnings surprise are often viewed as having been managed to meet or just beat analysts' earnings forecasts.

In line with Burgstahler and Dichev (1997), we identify earnings management cases as those for which the observed relative frequency of earnings that are just above (just below) an earnings threshold exceed (fall below) their theoretical values. For the purpose of this analysis, we divide the distribution of the earnings measure into "bins," with bin widths determined by the formula suggested by Degeorge et al. (1999). We test for the significance of the difference between the actual and theoretical frequency in a bin based on the procedure proposed by Burgstahler and Dichev (1997) whereby we calculate the standardized differences for the interval just below zero and the interval just above zero. Under the assumption of no earnings management, the expected number of observations in any given interval is equal to the average of the number of observations in the two adjacent intervals. If managers succeed in meeting the threshold, we would expect to find a shift of observations from the bins just below the earnings threshold to the bins just above that threshold.

A number of studies have expressed concerns regarding the effectiveness of this procedure to identify earnings management (e.g., Beaver et al. 2007; Durtschi and Easton

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[7] Total current accruals equal operating assets (current assets excluding cash and short-term investments) minus operating liabilities (current liabilities excluding the current portion of long-term debt). Estimating Regression (1) using total accruals and cash flows as defined in Regression (2) produces similar results to those from the estimation of Regression (1) as defined in the text (and tabulated in Section VI).

2005; Dechow et al. 2003). To increase the confidence that the cases occurring "just above" the identified thresholds are likely to represent earnings management, we follow Dechow et al. (2003) and investigate whether such cases have a higher proportion of positive unexpected discretionary accruals. To further refine the identification of earnings management cases, we examine the percentage of the positive unexpected accruals cases where these accruals "made the difference" in meeting or beating the threshold. That is, we focus on cases for which the magnitude of positive unexpected accruals was sufficiently positive, so as to turn what would otherwise be a loss (or a decline in earnings) into a small profit (or increase in earnings). We identify "expected" or "nondiscretionary" accruals using the modified Jones model (Dechow et al. 1995).[8]

Even though widely used in the earnings management literature, accruals models such as the modified Jones model are far from perfect in detecting earnings management. In particular, these models make certain assumptions about the functional relationship between accruals and activity measures such as sales or the level of plant, property, and equipment that, while plausible, may not strictly hold. The models further assume that the relationships between cash flows and accruals are linear, thus ignoring the asymmetry in the gain and loss recognition of accruals. We incorporate the improvement in accruals models proposed by Ball and Shivakumar (2006) in our estimation of the Dechow and Dichev (2002) and modified Jones (Dechow et al. 1995) models. Specifically, we augment both Regression (2) and the modified Jones model by adding an indicator variable, *DCFO* and an interactive variable *DCFO* * *CFO*, where *DCFO* receives the value 1 when $CFO < 0$, and 0 otherwise. Consistent with the results reported by Ball and Shivakumar (2006), the introduction of this proxy increases considerably the explanatory power of both accrual models.

Even with this improvement, the identification of earnings management through the various accrual models is very noisy. Patterns in earnings can be caused by a host of operational factors other than earnings management. Further, certain accruals are devoid of any earnings management implications. For example, abnormally high payments to suppliers or an abnormally high rate of collection from customers is captured by the modified Jones model as "abnormal" accruals, yet these may be unrelated to earnings management. Despite the absence of a proven methodology to detect earnings management, the reliance on a number of techniques, all of which are widely used in the earnings management literature, should enhance the validity of our results.

Conservatism

Another attribute of financial reporting is the extent of reporting conservatism, in the sense of a more timely recognition of economic losses as compared with the recognition of economic gains, resulting in a systematic undervaluation of the book value of the firm's equity relative to its economic value (Watts 2003; Givoly et al. 2007).

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[8] Specifically, we estimate the following regression cross-sectionally within each two-digit SIC code industry:

$$TACC_{i,t} = a_1{*}[1/TA_{i,t-1}] + a_2{*}[(\Delta REV_{i,t} - \Delta TR_{i,t})/TA_{i,t-1}] + a_3{*}[PPE_{i,t}/TA_{i,t-1}] + e_{i,t}$$

where *TACC* is total accruals defined as the difference between income from continuing operations and net cash flow from operating activities, excluding extraordinary items and discontinued operations, *TA* is the beginning-of-the-year total assets, Δ*REV* is the change in sales, *PPE* is the level of gross property, plant, and equipment and Δ*TR* is the change in trade receivables. For the years prior to 1988 when cash flow data are unavailable, we define total accruals as: Δcurrent assets − Δcurrent liabilities − Δcash + Δshort-term debt − depreciation and amortization expense, eliminating firm-year observations with "non-articulating" events (see Hribar and Collins 2002).

Public equity firms are likely to face a greater litigation risk because their stock prices are observable, making it easier for potential plaintiffs to both discover causes for lawsuits and establish damages (e.g., Kellogg 1984).[9] The higher level of litigation risk faced by public equity firms and their management can induce a greater degree of conservatism through an earlier recognition of losses (e.g., Skinner 1994, 1997).

In addition to a greater litigation risk, publicly owned firms may face a stronger demand by shareholders to reduce information asymmetry and monitor management's decisions through a more timely recognition of losses. These agency costs are less severe in privately held firms because their higher ownership concentration allows the resolution of any information asymmetry through "insider access." Further, the managers and shareholders of these firms often have a special relationship with top management (Fama and Jensen 1983), allowing for closer monitoring and performance evaluation. These firms thus have less need to rely on public disclosure and less incentive to incorporate economic losses into accounting income in a timely manner (Ball et al. 2000; Ball and Shivakumar 2005; Francis et al. 2005). So, while shareholder-related agency problems exist for both types of firms, they are likely to be more severe for publicly owned firms. Our expectation, therefore, is that public equity firms will exhibit a higher degree of reporting conservatism than will private equity firms.

Its potential role in reducing litigation risk or alleviating agency costs notwithstanding, there is an ongoing debate among standard-setters and academics regarding whether accounting conservatism is a desired property that enhances reporting quality. Watts (2003) argues that conservatism is desirable because it constrains managerial opportunistic behavior, offsets managerial biases with its asymmetrical verifiability requirements, and presents an efficient contracting mechanism. A similar view is expressed by Ball and Shivakumar (2005), who suggest that conservatism improves reporting quality by making financial statements more useful to parties contracting with the firm. Timely loss recognition deters managers from taking poor projects and investments and provides debt holders with more accurate information for loan pricing. Conservatism in this form is sometimes described as improving "transparency," since it reduces the information asymmetry between management and users of the financial statements (Ball et al. 2000; Bhattacharya et al. 2003).

The view that conservatism is a desirable property of accounting numbers is far from universal. For example, Penman and Zhang (2002) suggest that conservative accounting is undesirable because the hidden reserves that it generates facilitate earnings management, thus reducing the predictive ability of current earnings with respect to the firm's future performance. O'Connell (2007) suggests that conservatism may be beneficial for assessing stewardship but non-optimal from a valuation perspective. Contemporary accounting standard-setters clearly do not endorse conservatism as a desirable attribute of financial reporting. The FASB is quite explicit about the dangers inherent in reporting policies that lead to the consistent understatement of assets and earnings, warning that bias in estimating components of earnings "may mislead one group of investors to the possible benefit or detriment of others" (FASB 1984, paragraph 96, page 37). FASB's position implies that the best interest of the users is served by neutral reporting accompanied by appropriate disclosure of "the nature and extent of the uncertainty surrounding events and transactions reported to stockholders and others" (FASB 1984, paragraph 97, page 37). Wariness about the undesirable effect of unwarranted conservatism on the quality of financial reporting has also been expressed by the SEC (Levitt 1998). The very notion that there is some optimal degree of conservatism implies that "more" conservatism is not necessarily "better" for financial reporting.

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[9] This is particularly true in light of the acceptance by the courts in securities law litigation of the "fraud-on-the-market" doctrine (as articulated by the Supreme Court in *Basic, Inc., v. Levinson,* 485 U.S. 224 1988). This doctrine assumes that in an efficient market, the market price of a stock is a direct reflection of all material information known to the market relating to the issuer. The plaintiffs are thus presumed to rely on the disclosed information, without the need to prove such reliance.

In addition to conceptual concerns, there are empirical concerns regarding difficulties in identifying the presence of conservatism. These concerns stem from the fact that the observable items that are consistent with the presence and extent of conservatism are also consistent with other reporting attributes such as the presence of certain types of earnings manipulations, e.g., "big bath" (see Hanna 2003) or data characteristics such as frequency of losses (see Patatukas and Thomas 2009). Possibly reflecting these conflicting effects of conservatism on the quality of accounting numbers, Francis et al. (2005) fail to find a significant association between conservatism and the cost of equity capital, while finding strong support for the notion that accruals quality is priced.

Given the differing views on the link between conservatism and reporting quality, we provide results on the differential degree of conservatism between private and public equity firms without suggesting that this attribute necessarily connotes earnings quality.

We use the speed in which earnings reflect bad news as compared with good news as a measure of conservatism. This measure has been employed by a number of studies (e.g., Basu 1997; Ball and Shivakumar 2005). For the reasons explained earlier, we hypothesize that public equity firms will recognize economic losses (bad news) in a timelier manner than will those with private equity. To capture the differential timeliness of the earnings response to bad versus good news, we use a measure that captures the relative persistence of losses and gains. This measure is estimated as coefficient α_3 from the following piecewise linear regression:

$$\Delta NI_{i,t} = \alpha_0 + \alpha_1 D\Delta NI_{i,t-1} + \alpha_2 \Delta NI_{i,t-1} + \alpha_3 D\Delta NI_{i,t-1} * \Delta NI_{i,t-1} + \varepsilon_{i,t-1} \tag{3}$$

where ΔNI is the change in income (alternatively defined as including and excluding extraordinary items) from fiscal year $t-1$ to t, scaled by the beginning book value of total assets, and $D\Delta NI$ is an indicator variable set equal to 1 if ΔNI in the prior year is negative, and 0 otherwise.

Deferring the recognition of gains until their related cash flows are realized causes gains to be a "persistent" positive component of accounting income that tends not to reverse. An implication is that the coefficient α_2 is expected to equal 0. In contrast, the timely recognition of economic losses implies that they are recognized as transitory income decreases, resulting in subsequent earnings reversals and implying that $\alpha_2 + \alpha_3 < 0$. The hypothesis that economic losses are recognized in a more timely fashion than gains implies that $\alpha_3 < 0$.

Ball and Shivakumar (2005) develop an additional model to describe the differential timeliness of gain and loss recognition that relies on the correlation between accruals and contemporaneous cash flows, as follows:

$$ACC_{i,t} = \alpha_0 + \alpha_1 * DCFO_{i,t} + \alpha_2 * CFO_{i,t} + \alpha_3 * DCFO_{i,t} * CFO_{i,t} + \varepsilon_{i,t-1} \tag{4}$$

where ACC is total accruals in year t standardized by beginning-of-the-year total assets, CFO is cash from operations in year t adjusted for extraordinary items and discontinued operations and standardized by beginning-of-the-year total assets,[10] and *DCFO* is an indicator variable set equal to 1 if *CFO* is negative, and 0 otherwise. The role of accruals in mitigating the noise in operating cash flows would be reflected as $\alpha_2 < 0$. Conservatism, or the more timely recognition of losses, will lead to $\alpha_3 > 0$.

Consistent with Ball and Shivakumar (2005), we hypothesize that public equity firms are more likely to recognize economic losses on a more timely basis than private equity firms. As in the estimation of Regression (1), we account for the possible endogeneity in the choice to issue public or private equity using the Heckman (1979) two-stage procedure that involves the estimate of a PROBIT model and the determination of the inverse Mills ratio in the first stage and the inclusion of that ratio as a control variable in Regressions (3) and (4).

Another form of reporting conservatism, referred to as "unconditional conservatism," manifests itself in a systematic undervaluation of the firm's net assets (e.g., by the early expensing of costs, the deferral of revenues, or the creation of excessive loss provisions). Unconditional conservatism is observable by users of the financial statements, enabling them to adjust for its resulting bias. For these reasons, contracting-based demand for such a bias is unlikely (see Ball and Shivakumar 2005). Accordingly, we do not consider unconditional conservatism as an attribute demanded by investors. However, since previous studies document a negative association between conditional and unconditional conservatism (e.g., Beaver and Ryan 2005; Givoly et al. 2007; Roychowdhury and Watts 2007), we control for unconditional conservatism in testing for differences in conditional conservatism based on type of ownership.

V. SAMPLE

To form our sample, we first identify observations (firm-years) in the Compustat database (industrial, full coverage, and research) during the 26-year period from 1978–2003 that were likely to represent private equity, public debt firms using the following criteria: (1) the firm's stock price at year-end is unavailable, (2) the firm has debt (Compustat items #9 + #34) exceeding $1 million, (3) the firm is a separate, domestic company (and not an ADR or a subsidiary of another public firm), (4) the firm has at least $1 million in revenues, and (5) the firm has the data required to test the hypotheses for at least two years. We exclude firms in the financial industry (SIC codes 6000–6999) and other regulated industries (SIC 4800–4900).

The resulting initial sample consists of 2,817 distinct firms and 12,261 firm-year observations. We examine each firm in this sample to ensure that it had private equity and public debt in the identified time period, resulting in the elimination of about 80 percent of the firms that actually had public equity but met criterion (1) due to missing price data. We further eliminated some firms because their organizational and ownership structures made it likely that their reporting policies and management incentives would differ from those of private equity, public debt firms in general. Specifically, we eliminate 21 firms structured as cooperatives or subsidiaries of cooperatives (302 firm-year observations), three firms structured as limited partnerships (27 firm-year observations) and two government-owned firms (16 firm-year observations). The final private equity sample consists of 531 distinct firms and 2,519 firm-year observations.[11]

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[10] For the years prior to 1988, cash flow from operations is calculated as net income adjusted for depreciation and amortization as well as changes in working capital accounts.

To construct a sample of public equity firms, we identify firms in the same time period that met criteria (2) through (5). Similar to their private equity counterparts, we require that these firms have publicly traded debt exceeding $1 million. The presence of public debt in a given year is based on: (1) availability of S&P senior debt rating (Compustat item #280), (2) existence of debt debentures (#82), or (3) issuance of public debt according to the Mergent Fixed Income Securities database prior to the observation year with a maturity date beyond the observation year. Applying the above criteria results in a final sample of 3,954 distinct public equity firms (30,696 firm-year observations) with publicly traded debt.

Public equity and private equity firms may have different attributes in addition to ownership type that are likely to affect earnings quality, such as firm size. To control for the effect of these attributes, we use a matched-pair sample in some of our analyses. This sample is constructed by matching each private equity firm with a public equity firm in the same industry and of a similar size. To form this matched sample, we rank all firms in each sample (public equity and private equity) by their total assets at each year-end. We then partition the two samples into deciles to form ten firm-size portfolios. Each of the 2,519 firm-years in the private equity sample is then matched with an observation in the public equity firm sample drawn from the same size portfolio that (1) is the same year, (2) has the same three-digit SIC code, and (3) is closest in asset size to the private equity firm observation. The resulting matched pair sample consists of 538 matched pairs of private equity and public equity firm-years.

VI. RESULTS

Descriptive Statistics of the Private Equity, Public Debt Firm Sample

Table 1 provides descriptive statistics on the financial and other characteristics of the 531 firms (2,519 firm-years) in the private equity sample and the 3,954 firms (30,696 firm-years) in the public equity sample. Panel A shows the industry affiliation by type of owner. The private equity firms have a similar industry representation as the sample of public equity firms. Further, there is no particular industry clustering. However, as shown in Panel B, there are differences in the financial characteristics of the two samples of firms. Private equity firms are considerably smaller, less profitable, more leveraged, have a lower sales growth rate, are younger, and have a shorter operating cycle than publicly traded companies. However, in line with the notion that firms with a stronger financial position prefer and are capable of using the less costly and less restrictive public debt, note that private equity firms are generally financially sounder (with the exception of their sales growth) than those public equity firms that do not have public debt (firms shown in the rightmost column with private debt which are shown here for illustrative purposes but are not considered in the various tests). These characteristics are consistent with the economic reasons that prompt private equity firms to issue public debt: financing for a leveraged or management buyout.[12] Indeed, as Panel C of Table 1 indicates, private equity firms have a significantly higher

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[11] To identify private equity firms, we used data in the SEC filings on the EDGAR database (since 1993) and information on 10KWizard (prior to 1993), bankruptcy information from BankruptcyData.com, and other historical information in the Hoover's database and several news resources including Factiva, ProQuest, and Lexis-Nexis.

[12] Most of the private equity firms are owned by financial sponsors (e.g., Kohlberg, Kravis Roberts & Co.), management, or a combination of the two. These owner types suggest that the impetus for these firms to issue public debt was to affect a leveraged or management buyout.

TABLE 1
Descriptive Statistics of Private Equity and Public Equity Sample Firms

Panel A: Industry Affiliation of Sample Firms by Ownership Type

	Private Equity Firms		Public Equity Firms	
Industry (two-digit SIC codes)	**No. of Obs.**	**% of Sample**	**No. of Obs.**	**% of Sample**
Mining and Construction (10–14, 15–17)	18	3.4	318	8.0
Manufacturing I (20–29)	125	23.5	849	21.5
Manufacturing II (30–39)	164	30.9	1,439	36.4
Transportation and Public Utilities (40–49)	21	4.0	158	4.0
Retail and Wholesale Trade (50–59)	117	22.0	517	13.1
Services (70–89)	84	15.8	628	15.9
Other	2	0.4	45	1.1
Total Number of Firms	531	100.0	3,954	100.0

Panel B: Financial Characteristics of the Sample Firms by Ownership Type[a]

		Private Equity Firms with Public Debt	Public Equity Firms with Public Debt	Public Equity Firms with Private Debt
Number of Firms		531	3,954	10,673
Number of Firm-Year Observations		2,519	30,696	65,772
Total Assets ($ millions)	Mean	637	1,990	128
	Median	337	483	39
Total Sales ($ millions)	Mean	803	2,087	153
	Median	405	512	41
Leverage	Mean	67.2%	32.2%	25.5%
	Median	66.5%	29.5%	21.2%
Annual Sales Growth (%)	Mean	5.6%	9.9%	15.3%
	Median	4.6%	7.2%	8.5%
Return on Assets (%)	Mean	0.2%	3.2%	−3.7%
	Median	0.9%	4.3%	2.4%
Age of Firm (years)	Mean	8.9	19.9	10.2
	Median	4.0	18.0	7.0
Operating Cycle (days)	Mean	102	135	148
	Median	96	122	128
"Big" Auditor		93.8%	93.0%	76.6%

Panel C: Debt Rating of Private Equity and Public Equity Firms

	Private Equity Firms		Public Equity Firms	
S&P Debt Rating	**No. of Obs.**	**% of Sample**	**No. of Obs.**	**% of Sample**
BBB or Better	78	3.1	7,403	24.1
BB	241	9.6	3,471	11.3
B	1,001	39.7	2,890	9.4
C–CCC	100	4.0	318	1.0
D and Selective Default	9	0.4	187	0.6
Not Rated	1,090	43.2	16,427	53.6
Total Number of Firm-Years	2,519	100.0	30,696	100.0

(continued on next page)

TABLE 1 (continued)

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[a] The distribution of each variable is truncated at the extreme ±1% values.

Leverage = total debt/total assets;
Sales Growth = $[(\text{Sales}_t/\text{Sales}_{t-1}) - 1.0] * 100$;
Return on Assets = [net income/total assets] * 100;
Age = number of years since first appearance on Compustat;
Operating Cycle Days = receivable collection period plus inventory turnover (in days); (calculated as: [yearly average accounts receivables/(total revenues/360) + yearly average inventory/(cost of goods sold/360)]); and
"Big" Auditor = percentage of firms audited by one of the big national auditing firms.

concentration of S&P-rated debt in the BB–D range, as compared to public equity firms (53.6 percent and 22.4 percent, respectively).

Accrual Persistence

We assess the persistence of accruals through Regression (1). As noted earlier, we estimate this regression from the matched-pair sample described in Section V, controlling for the endogenous nature of the choice of ownership (public versus private) using the Heckman (1979) approach.[13]

Table 2 shows the coefficients from estimating an expanded version of Regression (1) that includes an indicator variable for ownership type (private equity or public equity) and, in Panel B, three control variables: leverage, growth, and firm size. The high level of significance of virtually all of the cash flow and accrual coefficients as well as the relatively high explanatory of the regression suggest that it captures well the relation between cash flows and accruals in the current year, and the operating income in the following year, for both groups of firms.

Our focus is on the coefficients on the cash flow and accrual components of operating income as well as the difference in these coefficients between private equity and public equity firms. If the accrual component of earnings causes earnings to be relatively less persistent than the cash flow component of earnings, then the coefficients on the accrual components of earnings will be smaller than those on the cash flow component of earnings. Using an F-test to test the equality of these coefficients (that is, testing whether $q_1 = q_2$ for public equity firms and whether $(q_1 + q_4) = (q_2 + q_5)$ for private equity firms), the hypothesis that they are equal is rejected for both types of firms.

More central to our hypotheses, note that private equity firms exhibit a greater persistence of both cash flows and accruals than do public equity firms. The incremental coefficient of cash flows (q_4) is positive and significant for both the regressions in Panels A and B of Table 2. The incremental coefficient of accruals (q_5), while positive for both regressions, is statistically significant only in Panel A. These results suggest that the quality of earnings of private equity firms, as captured by earnings persistence, is at least on par with, if not better than, that of public equity firms. This is consistent with the "opportunistic behavior" hypothesis, which suggests that financial reporting by public equity firms, because of capital market and managerial compensation incentives, is more susceptible to management intervention.

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[13] To obtain more efficient estimates for the cash flow and accrual variables, we also estimate Regression (1) augmented by variables that are likely to relate to future profitability (i.e., leverage, sales growth, and firm size).

TABLE 2
Persistence of Accruals Results from Estimating Regression (1)

$$OI_{t+1} = q_0 + q_1 * CF_t + q_2 * ACCR_t + q_3 PRIVATE + q_4 * PRIVATE * CF_t + q_5 * PRIVATE * ACCR_t + \varepsilon_t^{a}$$

	Panel A: Primary Variables		Panel B: Primary Variables and Additional Control Variables	
Variable	**Coefficient**	**t-statistic**	**Coefficient**	**t-statistic**
Intercept (q_0)	0.031	3.64***	0.015	0.82
CF_t (q_1)	0.797	26.49***	0.783	24.67***
$ACCR_t$ (q_2)	0.689	25.04***	0.638	18.14***
PRIVATE (q_3)	0.002	0.13	0.029	1.67*
PRIVATE * CF_t (q_4)	0.122	2.86***	0.132	3.10***
PRIVATE * $ACCR_t$ (q_5)	0.071	1.68*	0.061	1.48
LAMBDA	0.007	0.63	−0.005	−0.37
PRIVATE * *LAMBDA*	−0.015	−1.02	−0.023	−1.56
$LEVERAGE_t$			−0.035	−2.05***
$GROWTH_t$			0.084	2.93***
$SIZE_t$			0.003	1.26
Adjusted R^2	65.47%		67.97%	
Statistical Tests:				
F-test: $q_4 = q_5$	4.1**		7.6***	
F-test: $q_1 = q_2$	36.7***		36.1***	
F-test: $(q_1 + q_4) = (q_2 + q_5)$	77.1***		83.1***	
Number of Observations	896		865	

*, **, *** Significant at the 0.10, 0.05, and 0.01 levels, respectively.

[a] This regression is an expanded version of Regression (1) that incorporates the ownership type (public or private equity). It is estimated on the 538 firm-years in the matched-pair sample.

Variable Definitions:

OI_{t+1} = operating income after depreciation deflated by NOA in year *t*;
$ACCR_t$ = change in net operating assets from year *t*−1 to *t*, deflated by NOA in year *t*−1;
CF_t = operating profit after depreciation in year *t* divided by NOA in year *t*−1, minus $ACCR_t$;
$PRIVATE_t$ = indicator variable set to 1 for private equity firms and 0 for public equity firms;
$LEVERAGE_t$ = total debt divided by total assets at the end of year *t*;
$GROWTH_t$ = growth in total assets at the end of year *t*;
NOA = net operating assets computed as the book value of common and preferred equity plus long-term debt minus financial assets plus minority interest; and
LAMBDA = following Heckman (1979), a probit model is estimated with size (alternatively defined as total assets or sales), growth (in sales), leverage, profitability (operating income divided by net operating assets), the quick ratio, age, length of the operating cycle, and audit quality (an indicator for the big national accounting firms) as predictors; estimates of the probit model are used to compute an inverse Mills ratio for each firm; this ratio is included in Regression (1) as a control variable along with an interactive variable, *PRIVATE* * *LAMBDA*, to allow the coefficient to vary between the two groups of firms.

Estimation Error in the Accrual Process

The estimation error in the accrual process is gauged by the variability of the accruals that remain unexplained by Regression (2). We estimate this regression cross-sectionally within each industry separately for public equity and private equity firms. The separate estimation is needed because the basic relationship between accruals and cash flows may differ across the two types of firms.

The results from estimating this regression are provided in Table 3 for the full sample and for the 23 industries that had a sufficient number (at least 20) of both public equity and private equity firms to perform the analysis. The table presents the mean values of the estimation errors; the median values (not reported) are not significantly different from the mean values.

Examining the first row of results, note that for the overall sample both measures of variability, the standard deviation of the residuals shown in columns 1 and 3 (2.57 percent and 4.05 percent, respectively) and the ratio of this standard deviation to the standard deviation of total current accruals shown in columns 2 and 4 (47.15 percent and 61.91 percent, respectively), are significantly higher for public equity firms as compared with private equity firms. Both of these differences are statistically significant as shown in columns 5 and 6. Significant differences also exist at the industry level. As indicated by the industry differences shown in column 5, for the majority of industries (21 of 23), the standard deviation of the residuals from Regression (2) (the unexplained accrual variability) is higher for public equity firms than for those with private equity. Similar results characterize the ratio of the standard deviation of the regression residuals to the standard deviation of the total current accruals (the dependent variable), as shown in column 6. Both differences are statistically significant in 13 of the industries and are further confirmed by a number of nonparametric tests.

These results indicate that public equity firms exhibit significantly greater accrual variability as well as relative accrual variability (that is a higher ratio of the standard deviation of residuals from Regression (2) to that of total current accruals) than their privately owned peers operating in the same industries.

As a robustness check, we re-estimate Regression (2) including controls for size (alternatively defined as total assets or sales), growth in sales, leverage, profitability (RNOA), firm age, audit quality, and operating cycle. The main results remain intact.

Based on these results, we conclude that the accrual estimation of public equity firms is of a lower quality than that of private equity firms. This is consistent with our earlier findings on the persistence of accruals, and lends further support to the "opportunistic behavior" hypothesis.

Absence of Earnings Management

As explained in Section IV, we identify the presence of earnings management in the two groups of firms using two tests. The first test is based on the distributional properties of earnings around two earnings thresholds: zero earnings and zero earnings growth. We refer to this test as the "threshold analysis." The second test is based on the sign and magnitude of unexpected accruals of those observations that fall just above the two earnings thresholds.

The two compared groups of firms, those with private equity and those with public equity, may have different industry and operational characteristics that could potentially affect the earnings distribution around the thresholds and influence our inferences. To control for these characteristics, we conduct the threshold analysis using a matched-pair approach. Specifically, we match each of the private equity firm-years with an observation in the public equity firm sample that occurs (1) in the same year, (2) has the same three-digit SIC code, and (3) is closest in the probability of being private to the private equity firm observation. We estimate the probability of being private from the PROBIT model described in Section IV. The resulting sample consists of 1,193 pairs of matched firm-years.

TABLE 3
Mean Values of the Estimation Error of the Accrual Process Variability and Relative Variability of the Residuals from Regression (2)

$$TCA_t = \beta_0 + \beta_1 CFO_{t-1} + \beta_2 CFO_t + \beta_3 CFO_{t+1} + \beta_4 \Delta Rev_t + \beta_5 PPE_t + \beta_6 DCFO_t + \beta_7 DCFO_t * CFO_t + \varepsilon_t$$

	Private Equity Firms			Public Equity Firms			Difference between Public Equity and Private Equity Firms	
Industry (two-digit SIC)	No. of Obs.	(1) Std Dev. of Residuals	(2) Ratio of S.D.[a]	No. of Obs.	(3) Std Dev. of Residuals	(4) Ratio of S.D.[a]	(5) = (3) − (1) Difference in Std Dev. of Residuals	(6) = (4) − (2) Difference in Ratio of S.D.[a]
All Obs.	843	2.57%	47.15%	13,527	4.05%	61.91%	1.48%***	14.76%***
22	26	1.45	28.79	306	4.17	57.97	2.73***	29.18***
23	27	3.35	50.58	272	5.21	56.19	1.86	5.60
25	22	4.17	67.03	294	3.61	51.13	−0.56	−15.91
27	41	1.41	51.13	508	3.90	62.86	2.48***	11.72
28	61	2.60	58.53	1,878	4.22	72.44	1.62***	13.91**
30	25	2.24	32.24	496	3.76	63.39	1.53	31.15***
33	24	2.87	48.45	855	3.79	64.36	0.92**	15.90
34	39	2.59	44.18	627	4.46	62.84	1.87***	18.67**
35	43	3.06	47.23	1,716	5.12	67.38	2.05***	20.14***
36	27	2.97	44.65	1,710	5.08	64.07	2.11**	19.43**
37	59	4.23	68.40	951	4.14	60.63	−0.09	−7.76
42	36	1.75	34.61	170	2.44	58.53	0.69	23.92***
47	23	0.70	44.58	46	3.07	51.76	2.37***	7.17

(continued on next page)

TABLE 3 (continued)

Industry (two-digit SIC)	Private Equity Firms			Public Equity Firms			Difference between Public Equity and Private Equity Firms	
	No. of Obs.	(1) Std Dev. of Residuals	(2) Ratio of S.D.[a]	No. of Obs.	(3) Std Dev. of Residuals	(4) Ratio of S.D.[a]	(5) = (3) − (1) Difference in Std Dev. of Residuals	(6) = (4) − (2) Difference in Ratio of S.D.[a]
50	54	3.12	49.23	658	4.74	53.43	1.63***	4.20
51	27	2.07	23.23	392	4.00	50.17	1.93**	26.94***
53	20	3.21	45.58	383	3.61	53.43	0.40	7.85
54	60	1.76	50.08	312	2.35	61.70	0.59**	11.62*
58	65	3.11	54.51	286	3.48	70.62	0.37	16.11**
59	27	1.28	25.78	414	4.92	67.01	3.65***	41.22***
70	24	2.31	60.94	135	2.51	62.58	0.20	1.63
73	34	2.65	47.53	651	5.72	76.27	3.07***	28.74***
79	30	1.77	45.67	298	3.23	70.02	1.45**	24.35***
87	49	4.45	61.50	169	5.55	65.28	1.10	3.78

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*, **, *** Significant at the 0.10, 0.05, and 0.01 levels, respectively. Significance is determined using an F-test for equality of variances.

[a] Ratio of S.D. refers to the ratio of the standard deviation of the residuals from estimating Regression (2) (in the previous column) divided by the standard deviation of total current accruals.

Variable Definitions:

TCA_t = total current accruals computed as $\Delta CA - \Delta CL - \Delta Cash + \Delta STDEBT$; CA is current assets, CL is current liabilities, and $STDEBT$ is the current portion of long-term debt, and changes are computed from year $t-1$ to year t;

CFO_t = cash flow from operations computed as income from continuing operations in year t minus total accruals in year t where total accruals in year t equals TCA_t − depreciation and amortization in year t;

ΔRev_t = change in revenues from year $t-1$ to year t;

PPE_t = PPE (gross) in year t; and

$DCFO_t$ = indicator variable that is set to 1 if $CFO_t < 0$, and to 0 otherwise.

Table 4 presents the results of the threshold analysis using this matched-pair sample. These results are for earnings thresholds, where earnings are defined as income from continuing operations[14] and to intervals just above and just below the thresholds that correspond to two bin-widths using the bin definition employed by Degeorge et al. (1999).[15]

Panels A and B of Table 4 indicate that, for public equity firms, the actual frequency of cases just below (just above) the zero threshold of both earnings levels and earnings changes is lower (higher) than the expected frequency for that interval. The standardized difference between the expected and actual frequency, which under the null hypothesis would be distributed approximately Normal (0,1), is larger than 4.22 for the "just-above"

TABLE 4
Frequency Distribution of Earnings around Zero-Earnings and Zero-Earnings-Growth Thresholds[a]

Panel A: Zero Earnings Threshold (number of observations = 1,193 matched pairs)

	Private Equity Firms			Public Equity Firms		
	Frequency of Observations			Frequency of Observations		
Interval[b]	Actual	Expected[c]	Standardized Difference[d]	Actual	Expected	Standardized Difference
Just below zero	211	202	0.59	101	160.5	−4.34
Just above zero	285	209.5	4.89	244	182	4.22

Panel B: Zero Earnings Growth Threshold (number of observations = 792 matched pairs)

	Private Equity Firms			Public Equity Firms		
	Frequency of Observations			Frequency of Observations		
Interval	Actual	Expected	Standardized Difference	Actual	Expected	Standardized Difference
Just below zero	145	155	−0.78	117	140.5	−1.91
Just above zero	240	131.5	8.68	208	109	8.40

[a] In Panel A, the distribution of income from continuing operations in year t divided by total assets at the end of year $t-1$ (Income/Total Assets) is examined to assess potential earnings management around this threshold. In Panel B, the distribution of the change in income from continuing operations from year $t-1$ to year t divided by total assets at the end of year $t-2$ (ΔIncome/Assets) is examined.

[b] Following Degeorge et al. (1999), the bin width is calculated as $2 * 2(\text{IQR})n^{-1/3}$, where IQR is the sample inter-quartile range and n is the number of observations. The resulting bin width for the distribution of Income/Assets and ΔIncome/Assets is 0.028 and 0.020, respectively.

[c] The expected frequency in the interval is computed as the average of the number of observations in the two adjacent intervals.

[d] The standardized difference is the difference between the actual and expected frequency in the interval, divided by the standard deviation of the difference. The standard deviation of the difference is computed as the square root of $[n * P_i * (1 - P_i) + (1/4) * N * (P_{i-1} + P_{i+1})(1 - P_{i-1} - P_{i+1})]$, where N is the total number of observations and P_i is the probability that an observation will fall into interval i (see Burgstahler and Dichev 1997).

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[14] Although this definition most likely corresponds to the threshold that investors and management emphasize, we repeated the threshold analysis using both bottom-line net income and operating income. The findings were essentially the same.

[15] The determination of the width of the interval represents a tradeoff between fineness and precision and relies in part on the examination of the earnings distribution around the threshold. For further discussion of the "bin width" see Dichev and Skinner (2002, 1108). The use of single-bin-width intervals leads to very similar results.

regions. This finding, which is comparable with previous findings (e.g., Burgstahler and Dichev 1997), is consistent with upward earnings management in cases that otherwise would have fallen slightly short of the earnings thresholds.

While there is a significant excess concentration of cases "just above" the threshold for public and private firms, the shift from "just below" to "just above" the threshold is more pronounced for the public equity firms. In fact, there is not a significant underrepresentation of cases in the interval "just below" either of the examined thresholds for private equity firms, as indicated by the 0.59 and −0.78 standardized differences. This last finding makes the interpretation of the concentration of cases in the "just above" interval as a manifestation of earnings management less obvious for privately owned companies.

If earnings management takes the form of converting small losses to zero or small profits (or converting small earnings declines to the same earnings levels or small earnings increases), then we would expect to observe abnormal positive accruals in the interval just above the threshold. Table 5 shows the extent of abnormal accruals in that interval.

To more precisely pinpoint earnings management cases, we examine the percentage of the positive unexpected accruals cases for which these accruals' magnitude was sufficiently positive so as to turn what would have otherwise been a loss into a small profit (or to offset what would otherwise be an earnings decrease). Table 5 shows the proportion of cases in the "just above zero" interval for which the amount of unexpected positive accruals was larger than the amount by which reported income exceeded the threshold.

Two main findings emerge from this analysis. First, the percentage of cases with positive unexpected accruals in the interval just above the threshold is higher for the public equity firms than for the private equity firms. As shown on the first row of the results, 42.7 percent of the public equity observations classified as being in the "just above zero" interval of the earnings distribution contain unexpected positive accruals, while 39.5 percent of the private equity firms in this interval had unexpected positive accruals. This difference of 3.2 percent is not statistically significant. However, the difference pertaining to the "zero earnings change" threshold shown in Table 5, Panel B of 15.9 percent is statistically significant at the 1 percent level. The other finding that emerges from this analysis is that, among the cases with positive unexpected accruals that fall in the "just above zero" range, the frequency of cases in which unexpected accruals alone explain the excess of earnings over the threshold is larger for the public equity than for the private equity firms. To illustrate, for public equity observations, the magnitude of the unexpected accruals was sufficient to turn a loss into a profit for 28.4 percent of the cases and to turn an earnings decrease into an earnings increase for 46.1 percent of the cases. The corresponding percentages for the private equity firms are lower, 25.3 percent and 36.1 percent, respectively. The difference between the two groups in the percentage of cases where abnormal positive accruals were large enough to enable the firm to meet the threshold of zero or positive earnings growth is significant at the 1 percent level.

Table 5 also presents the magnitude of unexpected accruals (standardized by total assets) in the regions just above the zero threshold (as shown in the last line of each panel). The mean and median values of the public equity firms' unexpected accruals are more positive than those of the private equity firms. The overall tenor of the results in Table 5 is consistent with more pronounced earnings management for public equity firms, in line with the "opportunistic behavior" hypothesis.[16]

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[16] Repeating the analysis in Table 5 using different bin-widths or using the full sample produces similar results.

TABLE 5
Unexpected Accruals Behavior in the "Just above Zero" Interval[a]

Panel A: Zero Earnings Threshold (number of observations = 1,108 matched pairs)[b]

	Percentage of Observations[c]		
	Private Equity Firms	**Public Equity Firms**	**Difference in Percentage of Observations**
Percentage of Cases with Positive Unexpected Accruals[c] (out of all cases in the interval; no. of observations equals 225 and 261 for public equity and private equity firms, respectively)	39.5%	42.7%	3.2%
Percentage of Cases with Positive Unexpected Accruals Larger than the Excess over the Threshold[d] (out of all cases in the interval)	25.3%	28.4%	3.2%
Mean (Median) Unexpected Accruals Standardized by Total Assets	−0.43% (−0.72%)	−0.31% (−0.37%)	0.11% (0.35%)

Panel B: Zero Earnings Growth Threshold (number of observations = 740 matched pairs)

	Percentage of Observations[c]		
	Private Equity Firms	**Public Equity Firms**	**Difference in Percentage of Observations**
Percentage of Cases with Positive Unexpected Accruals[c] (out of all cases in the interval; no. of observations equals 191 and 216 for public equity and private equity firms, respectively)	41.7%	57.6%	15.9%***
Percentage of Cases with Positive Unexpected Accruals Larger than the Excess over the Threshold[d] (out of all cases in the interval)	36.1%	46.1%	10.0%***
Mean (Median) Unexpected Accruals Standardized by Total Assets	−0.58% (−0.78%)	0.63% (0.68%)	1.22%*** (1.46%)***

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*, **, *** Significant at the 0.10, 0.05, and 0.01 levels, respectively. Statistical significance is assessed using the t-test for differences in proportions.

[a] The interval just above (just below) zero is defined as the first positive (first negative) "bin" of the distribution. The bin widths are 0.028 and 0.020 for the distributions of Income/Total Assets and ΔIncome/Assets, respectively. Bin widths (BW) are determined by the formula: $2 * BW = 2 * 2(IQR)n^{-1/3}$, where IQR is the sample inter-quartile range and n is the number of observations.

[b] In Panel A, the distribution of income from continuing operations in year t divided by total assets at the end of year $t-1$ (Income/Total Assets) is examined to assess potential earnings management around this threshold. In Panel B, the distribution of the change in income from continuing operations in from year $t-1$ to year t divided by total assets at the end of year $t-2$ (ΔIncome/Assets) is examined.

[c] Unexpected accruals are derived from the cross-sectional modified Jones model (see Section IV). To control for the asymmetric recognition of gains and losses, we augmented the modified Jones model with the following independent variables: cash flow from operations in year t (CF_t), an indicator variable set to 1 if $CF_t < 1$ and 0 otherwise (DCF_t), and an interactive variable, $CF_t \times DCF_t$ (Ball and Shivakumar 2006).

[d] Cases where the threshold would not have been met in the absence of positive unexpected accruals.

Conservatism

As explained in Section IV, we measure conservatism by the differential timeliness of loss versus gain recognition, referred to in the literature as "conditional conservatism." We employ a measure proposed by Basu (1997) as well as an accrual-based model used by Ball and Shivakumar (2005) to assess the extent of such conservatism in the two groups of firms, and to further account for possible endogeneity. The results from estimating Regressions (3) and (4) are presented in Tables 6 and 7, respectively. The coefficients of interest in Regression (3) are those relating to the differential persistence of earnings declines versus earnings increases (a_3 for public equity firms and $a_3 + a_7$ for private equity firms) as well as the difference in this differential between these two groups of firms (a_7). We evaluate two earnings measures, net income and income from continuing operations.

Two main results are evident from the results presented in Table 6. First, consistent with previous research, financial reporting in general is conservative. Earnings increases are significantly more persistent than earnings decreases for both groups of firms. Both a_3 for public equity firms and $a_3 + a_7$ for private equity firms are negative and statistically significant for the earnings measures (a_3 of -0.537 for both earnings measures and $a_3 + a_7$ of -0.181 and -0.286 for the two earnings measures, respectively). Second, the extent of conservatism is greater for public equity firms as compared to that of private equity firms. The coefficient a_7, which indicates the excess persistence of earnings declines over earnings increases for public equity firms, is positive (0.357 and 0.251 for the two earnings measures, respectively) and statistically significant. Note that the coefficient of the inverse Mills variable (*LAMBDA*) is significant for both types of firms, suggesting the presence of, and appropriateness of controlling for, endogeneity.

Table 7 presents the results from Regression (4), in which accruals are regressed on contemporaneous cash flow variables. The coefficient of *CFO*, b_2, is significantly negative, indicating the strong role that accruals play in mitigating the noise in operating cash flows. The coefficient of the interactive variable *DCFO* * *CFO*, b_3, is significantly positive, suggesting that accruals are less negatively correlated with earnings in periods with "bad news" (as gauged by negative cash flows), suggesting a more timely incorporation of bad news as compared to good news in earnings. The coefficient of *PRIVATE* * *DCFO* * *CFO*, b_7, our variable of interest, is significantly negative at the 1 percent significance level, indicating a greater degree of conservative reporting by public equity firms as compared to private equity firms. These results confirm the findings regarding conditional conservatism reported in Table 6.

These results are consistent with those reported by Ball and Shivakumar (2005) for private and public companies in the U.K. We interpret this finding as indicating that public equity firms, because of their greater exposure to litigation risk and more severe agency problems, report more conservatively than do private equity firms in the sense of a more pronounced earlier recognition of losses relative to gains.

Prior research suggests that the extent of conditional conservatism may be related to the degree of unconditional conservatism. Since unconditional conservatism is also likely to be driven by certain firm characteristics (e.g., intensity of investment in intangible assets) that may vary between public and private firms, we control for unconditional conservatism in testing for differences in conditional conservatism between public and private firms. We accomplish this control by estimating for each firm the amount of its "hidden" reserves, as captured by the Q-score devised by Penman and Zhang (2002) that gauges the difference

TABLE 6
Differential Persistence of Profits versus Losses by Firm Ownership Type Summary Results for Expanded Version of Regression (3):

$$\Delta NI_t = a_0 + a_1 * D\Delta NI_{t-1} + a_2 * \Delta NI_{t-1} + a_3 * D\Delta NI_{t-1} * \Delta NI_{t-1} + a_4 * PRIVATE$$
$$+ a_5 * PRIVATE * D\Delta NI_{t-1} + a_6 * PRIVATE * \Delta NI_{t-1} + a_7 * PRIVATE * D\Delta NI_{t-1} * \Delta NI_{t-1} + e_t$$

			Earnings Measure			
			Net Income		Income from Continuing Operations	
Variable[a]	(Coefficient)	Predicted Sign under Conservatism and the "Demand" Hypothesis	Coefficient	t-statistic	Coefficient	t-statistic
Intercept	(a_0)	?	0.002	2.82***	0.002	2.72***
$D\Delta NI_{t-1}$	(a_1)	?	−0.012	−8.99***	−0.010	−8.48***
ΔNI_{t-1}	(a_2)	0	−0.067	−5.20***	0.008	0.65
$D\Delta NI_{t-1} * \Delta NI_{t-1}$	(a_3)	−	−0.537	−27.9***	−0.537	−27.7***
PRIVATE	(a_4)	?	−0.015	−2.91***	−0.015	−3.34***
$PRIVATE * D\Delta NI_{t-1}$	(a_5)	?	−0.005	−1.45	0.000	−0.08
$PRIVATE * \Delta NI_{t-1}$	(a_6)	−	−0.253	−7.04***	−0.194	−4.72***
$PRIVATE * D\Delta NI_{t-1} * \Delta NI_{t-1}$	(a_7)	+	0.357	6.30***	0.251	3.89***
$a_3 + a_7$[b]		−	−0.181	4.93**	−0.286	9.98***
LAMBDA	(a_8)		0.067	18.12***	0.055	17.59***
*PRIVATE * LAMBDA*	(a_9)		−0.055	−13.74***	−0.045	−12.88***
Adjusted R^2			10.44%		7.75%	
Number of Observations			21,501		21,441	

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*, **, *** Significant at the 0.10, 0.05, and 0.01 levels, respectively.

[a] Each variable is truncated at the extreme +/−1 percent values of its distribution.

[b] The F-test is used to test the hypothesis that $a_3 + a_7 = 0$.

Variable Definitions:

ΔNI_t = change in the earnings measure from year $t-1$ to year t, divided by total assets at the end of year $t-1$;

$D\Delta NI$ = indicator variable which is set to 1 if $\Delta NI_{t-1} < 0$, and 0 otherwise;

PRIVATE = indicator variable which is set to 1 for private equity firms and 0 for public equity firms; and

LAMBDA = see the description provided in Table 2.

TABLE 7
Conservatism Measured as the Relative Timeliness of Recognizing Losses versus Gains Summary Results for the Expanded Version of Regression (4):

$$\begin{aligned} ACC_t = {} & b_0 + b_1 * DCFO_t + b_2 * CFO_t + b_3 * DCFO_t * CFO_t + b_4 * PRIVATE \\ & + b_5 * PRIVATE * DCFO_t + b_6 * PRIVATE * CFO_t \\ & + b_7 * PRIVATE * DCFO_t * CFO_t + e_t \end{aligned}$$

Variable[a]	(Coefficient)	Predicted Sign	Coefficient	t-statistic
Intercept	(b_0)	?	0.002	1.82*
$DCFO_t$	(b_1)	?	0.008	3.90***
CFO_t	(b_2)	−	−0.442	−58.12***
$DCFO_t * CFO_t$	(b_3)	+	0.103	3.63***
PRIVATE	(b_4)	?	−0.071	−15.73***
$PRIVATE * DCFO_t$	(b_5)	?	−0.018	−2.41**
$PRIVATE * CFO_t$	(b_6)	?	−0.068	−2.28**
$PRIVATE * DCFO_t * CFO_t$	(b_7)	−	−0.340	−2.83***
$a_3 + a_7$[b]		?	−0.237	4.12**
LAMBDA	(b_8)	?	0.127	41.67***
*PRIVATE * LAMBDA*	(b_9)	?	−0.096	−23.21***
Adjusted R^2			26.12%	
Number of Observations			21,405	

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*, **, *** Significant at the 0.10, 0.05, and 0.01 levels, respectively.

[a] Each variable in the table is truncated at the extreme +/−1 percent values of its distribution.

[b] The F-test is used to test the hypothesis that $a_3 + a_7 = 0$.

Variable Definitions:

ACC_t = total accruals divided by total assets at end of year $t-1$; for years 1988–2003, total accruals equal [income before extraordinary items − net cash flow from operating activities + extraordinary items and discontinued operations] divided by total assets at end of year $t-1$. For years prior to 1988 total accruals equal [change in current assets in year t − change in current liabilities in year t − change in cash and cash equivalents in year t + change in current maturities of long-term debt and other short-term debt included in current liabilities in year t − depreciation and amortization expense in year t]. Firm-year observations with the following events were eliminated: (1) a merger or acquisition, (2) discontinued operations where the absolute value of the dollar impact exceeded $10,000; (3) a gain or loss on foreign currency translations where the absolute value of the dollar impact exceeded $10,000; see Hribar and Collins (2002) for a discussion of this approach;

CFO_t = cash flow from operations divided by total assets at the end of year $t-1$; for years 1988–2003, net cash flow from operating activities was used. For years prior to 1988, net cash flow from operating activities was estimated as: [funds from operations − change in current assets in year t + change in cash and cash equivalent in year t + change in current liabilities in year t − change in current maturities of long-term debt and other short-term debt included in current liabilities in year t]. All variables are divided by total assets at end of year $t-1$; see Xie (2001) for a discussion of this approach;

$DCFO_t$ = indicator variable set to 1 if $CFO_t < 0$, and 0 otherwise;

PRIVATE = indicator variable set to 1 for private equity firms and 0 for public equity firms; and

LAMBDA = see the description provided in Table 2.

in the ratio of the hidden reserves to total assets between the firm and its industry.[17] Controlling for unconditional conservatism, we continue to find a greater degree of conditional conservatism among public firms.

Can Conservatism Co-Exist with Earnings Management?

The findings discussed above suggest that public equity firms report more conservatively than their private equity counterparts, consistent with the finding of Ball and Shivakumar (2005) for U.K. firms. At the same time, public equity firms have a greater propensity to manage income, in line with the results of U.S. firms in the banking industry reported by Beatty et al. (2002). The question arises as to whether these two findings are contradictory.

Empirically, the finding of greater conditional conservatism, that is, a more pronounced asymmetric response of earnings to bad versus good economic news, is not necessarily inconsistent with the presence of income-increasing earnings management. First, note that earnings management is situational, or episodic: it is likely to occur in situations in which unmanaged earnings would fail to meet a reporting objective considered by management to be important (such as meeting analysts' earnings forecasts, loss avoidance, or earnings decrease avoidance). If the company's accounting is generally conservative, earnings management will only temporarily interrupt the observed reporting pattern, and is therefore unlikely to render the reporting pattern to be aggressive overall. Second, earnings management typically involves small magnitudes of earnings, both because the positive accruals needed for such an activity are in short supply (Barton and Simko 2002) and because large-scale earnings management is more easily detected and undone by investors. This is why studies on earnings management consider likely earnings management cases to be those where earnings are "just above" the earnings threshold. Because of their small magnitude, the presence of instances of earnings management in the data is unlikely to obscure the presence, if any, of the much more prevalent phenomenon of reporting conservatism.

Note also that the presence of *unconditional* conservatism tends to increase the likelihood of earnings management because the reserves of accruals generated by this type of conservatism make it easier to engage in earnings management in case of the "need" to beat a particular threshold (Penman and Zhang 2002).

To more directly examine the coincidence of earnings management and conservative reporting, we perform two tests. First, we test the extent of earnings management (per our analysis in Table 4) for subsamples of firms that exhibit different levels of conditional conservatism. Specifically, we rank firms by their degree of conditional conservatism (as captured by the coefficient α_3 in Regression (3)) and partition them into quintiles. The results (not tabulated) show no difference in the presence of earnings management across the conservatism quintiles.

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[17] Following Penman and Zhang (2002), the firm's hidden reserves are estimated by the C-score, which equals the sum of the value of the LIFO reserve, the research and development reserve (calculated as the estimated value of R&D assets that would have been reported on the balance sheet had R&D not been expensed), and the advertising reserve (estimated as the brand assets created by advertising expenditures). This sum is standardized by the value of net operating assets at the end of the prior year as previously defined. To determine the (hypothetical) R&D assets, we "capitalize" the annual R&D expenditures and amortize them using the sum-of-the-years' digits method over a five-year period. Similarly, we estimate the advertising reserve by "capitalizing" advertising expenses and amortizing them using the sum-of-the-years' digits method over a two-year period. The Q-score is calculated as the firm's C-score minus the median C-score of the firm's industry where industry is defined based on the two-digit SIC code. We rank firms by their Q-score and define an indicator variable that partitions the firms based on whether they are above or below the median of the Q-score distribution. Through the use of interactive indicator variables, we obtain the equivalent of a separate estimation of Regressions (3) and (4) for firms below and above the median Q-score value.

The second test involves a simulation analysis in which we inject earnings-increasing components into earnings data generated by a process that conforms to conditional conservatism. Specifically, conservative behavior is introduced by modeling earnings as responding promptly and proportionally to contemporaneous negative economic shocks but not to contemporaneous positive economic shocks.[18] Earnings management is introduced to the simulation by adding a positive increment to unmanaged earnings whenever their value is just below zero, so as to avoid a loss. The results (not tabulated) show that the observed level of conservatism does not decrease when earnings management instances are present.

VII. CONCLUDING REMARKS

The findings of this study illustrate that both management incentives and demand by investors for earnings quality are important factors that shape the financial reporting of firms. Public ownership of the firm's equity exposes management to investors' demand for reporting quality. This demand, which is expressed by investors in the form of the regulatory and legal environment in which public equity firms operate, could lead to higher reporting quality (the "demand" hypothesis). At the same time, our findings support the notion that management of firms whose equity is publicly traded has stronger incentives to manage earnings, thus reducing the reliability and usefulness of financial reports. That is, the findings are more consistent with the "opportunistic behavior" hypothesis. We further find that public equity firms report more conservatively than privately held firms, although this result does not necessarily imply a higher quality of reporting for the former group of firms.

While we use the current methodology to measure the various proxies for earnings quality, these proxies are still subject to potential measurement errors. However, we are unaware of any systematic bias that these measurement errors introduce in our comparative analysis of the effect of ownership type on earnings quality. Overall, while public equity and private equity firms differ along various quality and financial attributes dimensions, neither type of firm dominates the other as having the higher quality of financial reports. Unless weights are assigned to different dimensions of earnings quality and attributes, one cannot conclude that the public listing of a firm's equity necessarily improves the quality of its financial reporting.

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[18] The simulation analysis uses the approach described in Givoly et al. (2007, Appendix A) for single-event periods. The degree of conservatism is assessed by the coefficient of the interactive indicator variable in the regression of return on earnings and an interactive variable of the return and an indicator variable that receives the value of 0 (1) when the return is not negative (is negative) (see Basu 1997). The estimates are based on earnings data generated by 100,000 iterations of an earnings generating process whereby earnings respond immediately to negative, but not positive, economic shocks. In order to determine whether earnings management alters the observed level of conditional conservatism, the simulation is conducted twice, once where earnings management in the form of loss avoidance exists and a second time where this form of earnings management does not exist.

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Information Content and Value Relevance of Depreciation: A Cross-Industry Analysis

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ABSTRACT: Funds from Operations (FFO) is the prevailing performance measure in the Real Estate Investment Trust (REIT) industry. However, prior studies are inconclusive about the superiority of FFO over GAAP net income. Because depreciation is the largest reconciling item between FFO and net income, we examine the information content and value relevance of depreciation for both the REIT and non-REIT industries and report the following findings. First, accumulated depreciation is value-relevant for the REIT industry, whereas accumulated depreciation has little value relevance for comparably capital-intensive non-REIT industries. Second, accounting depreciation deviates from economic depreciation to a greater extent for REITs than for non-REIT industries. Third, accumulated depreciation has predictive ability for future revenues for REIT firms, but not for non-REIT firms. Finally, only the REIT industry displays all of these properties. In sum, evidence supports the REIT industry's assertion that GAAP depreciation consistently exceeds economic depreciation and that book value of assets is systematically understated.

Keywords: *funds from operations; REIT; value relevance; accounting depreciation; economic depreciation.*

Data Availability: *Data used in this study are available from public sources.*

I. INTRODUCTION

Generally Accepted Accounting Principles (GAAP) require that depreciation expenses be charged to all fixed assets based on the estimated economic life of properties, including real estate properties owned by Real Estate Investment Trusts (REIT). Managers of the REIT industry frequently argue against depreciation provisions, claiming that real estate properties typically appreciate, not depreciate, over time. Because

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of this unique characteristic, REIT operators are concerned that recording depreciation and amortization expenses (hereinafter referred to as depreciation expense) distorts both assets and income.

Allegedly to better communicate performance and true value to investors, the REIT industry emphasizes funds flow information, rather than conventional GAAP income. In 1991, the trade organization of REIT firms, the National Association of Real Estate Investment Trusts (NAREIT), issued its first formal definition of Funds from Operations (FFO). Since its adoption, FFO has become the industry standard for performance measurement. In particular, most REITs disclose and highlight FFO information in press releases and 10-K filings, and analysts treat FFO per share and price/FFO multiples as the equivalent of EPS and P/E ratios.[1] By 2001, all major Wall Street analysts supplied FFO forecasts to Thomson Financial/First Call for REIT firms (Smith 2001).

The alleged superiority of FFO over net income (NI) for performance evaluation and security valuation has not been borne out in academic studies, however. Results reported by Fields et al. (1998), Gore and Stott (1998), and Vincent (1999) are inconclusive regarding the superiority of FFO relative to alternative income measures. A more fundamental issue, articulated by Skinner (1999), is that empirical findings are difficult to interpret without understanding the economic rationale behind the industry's claim of FFO's superiority.

According to NAREIT, a major drawback of GAAP net income is that GAAP depreciation misrepresents property values and thus potentially misleads investors. Because depreciation is the largest reconciling item between FFO and NI, the usefulness of FFO largely depends on the value relevance of depreciation expense (Vincent 1999). The purpose of this study is to examine the information content and value relevance of depreciation and accounting gains/losses for both REIT and non-REIT industries.

We build on prior research by Fields et al. (1998), Gore and Stott (1998), and Vincent (1999) but offer a number of distinguishing features. First, we offer an economic rationale for why the information content and value relevance of depreciation differ between REITs and other companies. In brief, we postulate that investors treat REIT properties as financial assets (or assets held for investment purposes), rather than assets held for operating (use) purposes. Therefore, in contrast with prior empirical investigations that focus on the REIT industry alone, we compare the value relevance of depreciation for REITs with the value relevance of other comparably capital-intensive non-REIT industries. Such comparisons not only indicate whether investors treat REIT assets differently, but also illustrate the implications of accounting depreciation in more general settings.

Second, we use both stock price and non-stock price data to investigate whether the information content and value relevance of REIT depreciation differ from the information content and value relevance of other industries. In contrast with prior research (Fields et al. 1998; Gore and Stott 1998; Vincent 1999), our analyses focus on whether investors discriminate among potentially relevant information, rather than whether FFO or NI is the superior performance measure.

Tests in the capital market setting employ both a stock returns model (e.g., Vincent 1999) and a levels (price) model (Kothari and Zimmerman 1995; Francis and Schipper 1999). The former is used to assess the information content of depreciation *expense* and the latter to assess the value relevance of *accumulated depreciation.* We employ both approaches because the low explanatory power and error-in-variables problem from the returns model potentially bias parameter estimates toward zero (Brown et al. 1987; Lev 1989; Kothari 1992; Kothari and Zimmerman 1995). By contrast, as the balance sheet at any given time captures the cumulative impact of *all past* understatements of assets, the price-level model focusing on the balance of accumulated depreciation offers a powerful test of the claim that GAAP depreciation misstates REIT performance and value.

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[1] Based on the I/B/E/S, the proportion of REITs that received forecasts exclusively using FFO (EPS) were 89 percent (6.6 percent), 86 percent (3.1 percent), 97.5 percent (0.8 percent), and 64.6 percent (2.7 percent) in 1998 through 2001, respectively (Downs and Güner 2006).

Tests in the non-stock market setting include an examination of whether accounting depreciation deviates more substantially from *economic* depreciation (Hotelling 1925; Beaver 1998) for REITs than for other firms. We theoretically and empirically demonstrate that the extent to which accounting depreciation overstates (understates) economic depreciation can be assessed by the extent of a positive (negative) association between accounting accumulated depreciation and gains/losses upon property disposals. We also examine whether accumulated depreciation predicts future revenues and whether it correlates with the net asset value (NAV) assessed by REIT analysts.

We report a number of key findings. First, consistent with Gore and Stott (1998), conventional tests of association between stock returns and income statement components indicate that REIT depreciation expense has little information content above and beyond FFO. Evidence indicates, however, that depreciation expense also has little incremental information content for other capital-intensive firms. Collectively, these results do not support a definitive conclusion that investors view depreciation expense as economically insignificant, as insignificant associations are potentially attributable to the low statistical power of the returns model.

Second, tests of associations between the *level* of stock price and balance sheet components—which we argue are more convincing tests of determining the value relevance of depreciation—demonstrate that REIT *accumulated depreciation* is value-relevant conditional on reported GAAP net income and net book value of assets. This result is consistent with both the REIT industry's position and the evidence that depreciation expense has little information content in the returns specification. By contrast, accumulated depreciation has little value relevance in price-level regressions of non-REIT firms. Analyses using the NAV data, which are available for a subset of the REIT sample, confirm these interpretations. More specifically, accumulated depreciation displays incremental value relevance in a specification where NAV is the dependent variable.

Third, a test of association between gains and losses from sales and accumulated depreciation of property disposals reveals that GAAP depreciation more substantially *exceeds* economic depreciation for REITs than for other capital-intensive firms.

Finally, we report evidence that accumulated depreciation predicts future revenues for REIT firms, but not for other firms. Such a predictive ability supports a characterization that some REIT assets, although still productive, are omitted from the balance sheet due to over-depreciation.

In summary, evidence using both the capital market and non-stock market data is consistent with the REIT industry's position that GAAP depreciation systematically deviates from economic depreciation of REITs' assets and that the corresponding book values are similarly understated.

Section II provides background information regarding the REIT industry and the FFO measure. Section III reviews the prior literature, and Section IV explains the research design and hypotheses. Section V describes the sample selection. Section VI reports the empirical results. Finally, Section VII draws conclusions.

II. REAL ESTATE INVESTMENT TRUST AND FUNDS FROM OPERATIONS

There are three types of REITs—equity, mortgage, and hybrid. Equity REITs invest in commercial real estate properties. Mortgage REITs invest in loans and other obligations secured by real estate properties. Hybrid REITs are combinations of both equity and mortgage REITs. This study excludes mortgage REITs because they do not own real estate properties (Vincent 1999).[2]

To overcome the alleged problem that "GAAP historical cost depreciation of real estate assets is generally uncorrelated with changes in the value of those assets whose value does not diminish predictably over time" (National Association of Real Estate Investment Trusts [NAREIT] 2002b, 3), NAREIT adopted the term "Funds from Operations" in 1991. Later in 1999, NAREIT implemented a sweeping change in the operational definition of FFO. The revision, effective January 1, 2000, intends to improve consistency and comparability of FFO by prohibiting exclusion of nonrecurring items, except those defined as "extraordinary items" under GAAP, and gains and losses from sales of depreciable operating properties (NAREIT 1999). Furthermore, operating results from properties held for sale, sold, or transferred should be included in FFO (NAREIT 2002b). This provision constrains the ability to manipulate FFO by arbitrarily earmarking poorly performing properties as a part of nonrecurring operations or as properties held for sale (Baik et al. 2008).[3]

According to NAREIT, the new definition of FFO is "net income (computed in accordance with generally accepted accounting principles), excluding gains (or losses) from sales of property, plus depreciation and amortization, and after adjustments for unconsolidated partnerships and joint ventures. Adjustments for unconsolidated partnerships and joint ventures will be calculated to reflect funds from operations on the same basis" (NAREIT 2002b, 2).

As explained by Vincent (1999) and Baik et al. (2008), FFO is a unique performance measure that cannot be reconstructed precisely by disaggregating reported financial data.[4] For example, NAREIT specifies that only depreciation and amortization uniquely significant to the real estate industry be added back to derive FFO. Nevertheless, because REIT firms do not separately disclose real-estate-related depreciation expenses on income statements, it is difficult for investors to calculate accurately the depreciation expense to be added back to FFO. In addition, because the FFO for unconsolidated partnerships and joint ventures are calculated "on the same basis" as the parent company, reconstructing FFO is impossible without access to the annual reports of unconsolidated subsidiaries. Even so, major differences between net income and FFO can be estimated with reasonable precision. Baik et al. (2008), using the approach of Vincent (1999) to reconstruct FFO from annual report data, report a mean (median) estimation error of 8.2 percent (6.6 percent). In this study, the objective is to examine how investors discriminate among potentially relevant *components* of income, rather than whether investors can distinguish one income measure from another by using publicly available information. Therefore, we construct FFO using the indirect method proposed by Gore and Stott (1998) and Vincent (1999) and examine the information content of three major income components—FFO, depreciation, and gains/losses.

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[2] We identify whether a REIT is an equity, hybrid, or mortgage REIT, based on the REIT index constituent file posted on the website of NAREIT (http://www.reit.com).

[3] Baik et al. (2008) report evidence suggesting that, relative to the old FFO definition, the new FFO reporting guideline mitigates managers' opportunistic reporting, and investors perceive less manipulation and greater reliability from the new FFO measure.

[4] Owing to this reason, the REIT industry lobbied vigorously to retain the right to disclose FFO when faced with potential regulatory constraints. For example, on November 4, 2002, the Securities and Exchange Commission solicited comments for a proposed new rule (33–8145) prohibiting the use of non-GAAP per share information in earnings releases and other similar public announcements. NAREIT and its member firms filed comment letters strongly opposing the proposed rule. According to the NAREIT (2003) *Financial Reporting Alert* dated January 15, the REIT industry filed more comments than any other impacted sector.

Notice that FFO differs from pro forma earnings. The latter normally includes recurring depreciation and amortization because it allegedly portrays core operating results better by removing transitory items (Halsey and Soybel 2002). Nevertheless, many firms also exclude depreciation and amortization (Bhattacharya et al. 2004), typically arguing that these are noncash charges and advocating the use of EBITDA. By contrast, FFO includes nonrecurring operations but *always* excludes recurring GAAP depreciation expense. FFO also differs from cash flow. Unlike operating cash flow, which excludes all accruals (Dechow 1994), FFO excludes only long-term accruals.

III. PRIOR LITERATURE

Prior studies investigating the role of depreciation expense either ignore REITs (e.g., Barth et al. 2001) or do not examine the information content of depreciation (Fields et al. 1998; Tsang 2006). Only Gore and Stott (1998) directly examine the information content of real estate depreciation for 597 REIT observations for the period 1991–1996. They find that FFO and gains on sales of properties are significantly related to stock returns, while depreciation is not. However, Vuong's (1989) likelihood ratio test indicates that neither FFO nor NI has greater explanatory power over the other. Gore and Stott (1998) use levels, instead of changes, in FFO, depreciation expense, and gains in the returns specification. As pointed out by Kothari and Zimmerman (1995), the error-in-variable problem from using the levels variables can bias the slope coefficients toward zero in a returns model. Thus, their results are inconclusive about the superiority of FFO over NI.

Using data for 138 publicly traded REITs during 1994–1996, Vincent (1999) presents a comprehensive analysis of information content and explanatory power of FFO *vis-à-vis* other popular measures of firm performance, namely, EPS, CFO, and EBITDA. Vincent (1999) employs both levels and changes of the performance measures to mitigate the concerns discussed above. Using annual data for 181 REITs, Vincent (1999) finds that FFO per share, but not EPS, is significantly associated with stock returns. On the other hand, using quarterly data (n = 850), EPS, but not FFO per share, is significantly related to stock returns. The Vuong (1989) tests indicate that neither FFO nor NI dominates the other.[5]

Fields et al. (1998) compare the usefulness of FFO and GAAP NI using hand-collected FFO information for 201 REIT firm-year observations for the period 1991–1995. They find that FFO better predicts one-year-ahead FFO and operating cash flows, and that GAAP NI explains significantly greater variation in one-year-ahead NI. Nonetheless, FFO and NI do not differ in terms of explaining stock prices.

To summarize, previous studies are inconclusive about the relative superiority of FFO over GAAP NI. Our approach goes beyond previous studies, however. First, we propose an economic rationale for how information content and value relevance of depreciation for REITs differ from the information content and value relevance of other industrial companies. Second, we examine the value relevance of depreciation and accumulated depreciation using both a returns model and a levels model. Third, using non-stock price data, we explore whether accounting depreciation exceeds economic depreciation and whether accumulated depreciation predicts future benefits (revenues). Finally, we investigate whether stock price and non-stock price properties of depreciation and accumulated depreciation differ between REITs and comparably capital-intensive non-REIT firms.

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[5] Baik et al. (2008) study the change in the reliability of FFO disclosure after NAREIT redefined FFO. Downs and Güner (2006) examine the forecast properties of FFO and NI. We do not review these studies as they are not directly related to the issues addressed in this study.

IV. HYPOTHESES AND RESEARCH DESIGN

Noting that the core controversy of the alleged superiority of FFO over NI concerns the treatment of depreciation,[6] several arguments support a preference of FFO over NI. First, compared to other types of fixed assets, such as machinery and equipment, real estate properties are less subject to obsolescence and impairment. In fact, REIT properties are arguably subject to economic appreciation, rather than economic depreciation.[7] On the balance sheet, REIT properties are more than 80 percent of total assets, whereas depreciable property is only 30 percent of assets for a typical publicly traded non-REIT company. If accounting depreciation distorts underlying economic values, as asserted by the REIT industry, then the effects on performance and value are greater for REIT firms than for other firms.

Second, properties owned by non-real estate companies, such as factories or machineries, are usually constructed for *use*, rather than for *sale*. For an operating company, the value of a specific real estate property is determined not by its stand-alone value, but by the combined value of *all* operating assets based on *aggregate* future cash flows for the entire firm. As a result, the resale value of an individual property is of little relevance in most situations. Furthermore, because these properties are typically acquired or constructed to serve firm-specific needs, they tend to have limited liquidity on the resale market. On the other hand, most properties held by REITs (typically, residential and office buildings, shopping centers, and hotels) are stand-alone business units with operating cash flows and fair market values that can be estimated with reasonable precision. As a result, REIT properties tend to have relatively high liquidity.

For these reasons, we postulate that REIT property resembles a long-term asset held for investment purposes—similar to a financial asset—with rental income analogous to interest income in financial investments, and property value appreciation similar to unrealized gain (economic appreciation) on the financial asset. Accordingly, we anticipate that (1) REITs' depreciation expense has little information content for income statement purposes; (2) accumulated depreciation of REITs has greater value relevance than accumulated depreciation of other operating companies; (3) accounting depreciation for REITs deviates from economic depreciation to a greater extent than for non-REITs; and (4) property value omitted from the balance sheet in the form of accumulated depreciation has greater predictive ability for future revenue for REITs than for non-REITs.

Hypotheses on Depreciation Expense

Building on Gore and Stott (1998) and Vincent (1999), the first hypothesis tests whether depreciation expense explains stock returns above and beyond the FFO information. The hypothesis, stated in the null, is as follows:

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[6] Untabulated evidence shows that the mean FFO per share and NI per share for REITs are \$2.368 and \$1.244, respectively. Among the reconciling items of \$1.124, the average depreciation per share is \$1.273, which is about 8.6 times the average gain/loss per share (−\$0.148).

[7] According to the *Property Index Detailed Quarterly Performance Report, 3rd Quarter*, issued by the National Council of Real Estate Investment Fiduciaries (2006), the fair market value of commercial real estate properties, on average, appreciated 1.98 percent annually during the 29-year period from 1977 through the third quarter of 2006.

H1a: There is no association between stock return and depreciation expense.

The next hypothesis considers whether the stock market properties of depreciation expense differ between REIT firms and non-REIT firms:

H1b: The association between stock return and depreciation expense does not differ between REIT and non-REIT firms.

To test H1a, we decompose net income as NI = FFO − depreciation + gain (or loss), following Gore and Stott (1998). We then estimate the following specification:

$$AR_{i,t} = \alpha_0 + \alpha_1 \Delta FFO_{i,t} + \alpha_2 \Delta DEP_{i,t} + \alpha_3 \Delta GAIN_{i,t} + \alpha_4 \lg MV_{i,t-1} + \alpha_5 BM_{i,t-1} + \sum_{i=2000}^{2004} \alpha_i Y_i + \varepsilon_{i,t}, \tag{1}$$

where $AR_{i,t}$ is firm i's market-adjusted buy-and-hold stock return inclusive of dividend, measured from the beginning of the fourth month of the current fiscal year until the end of the third month of the following fiscal year.[8] Table 1 contains definitions of all variables employed in this study.

The independent variables ΔFFO, ΔDEP, and $\Delta GAIN$ are, respectively, year $t-1$ to year t changes of *FFO*, depreciation expense, and gain (or loss), scaled by the beginning of the year stock price (Christie 1987; Vincent 1999). Lg*MV* and *BM* are the natural log of market capitalization and the book-to-market ratio at the beginning of year t, respectively. Prior literature shows that both firm size and book-to-market ratio are significant factors explaining excess returns.

To control for residual serial correlation, we employ Huber-White standard errors clustered by firm (Petersen 2009). To consider potential cross-sectional dependence of residuals (Sefcik and Thompson 1986; Bernard 1987; Froot 1989; Petersen 2009), we include time indicator variables, for each year from 2000 to 2004. Allowing the intercept to vary across years controls for time-specific macroeconomic effects (Aboody et al. 1999).

For H1a, the null hypothesis is $\alpha_2 = 0$, which implies that depreciation expense has little information content beyond *FFO*. Notice that such a result also implies that depreciation expense *does* have information content beyond *NI*, in the sense of having to "add back" depreciation expense.

Moreover, the following coefficient restrictions are of interest. First, noting that $\Delta NI = \Delta FFO - \Delta DEP + \Delta GAIN$, evidence in favor of the restriction $\alpha_1 = -\alpha_2 = \alpha_3$ implies that the three earnings components can be collapsed into a single component, *NI*. If so, then *NI* is a comprehensive summary measure for investors. On the other hand, evidence in favor of $\alpha_2 = \alpha_3 = 0$ (i.e., estimates for ΔDEP and $\Delta GAIN$ are zero) implies that *FFO*, not *NI*, is the superior summary measure and that *DEP* and *GAIN* lack incremental information content beyond *FFO*. Furthermore, it can be shown that if $\alpha_2 = \alpha_3 = 0$ holds *and* $\alpha_1 = -\alpha_2 = \alpha_3$ is rejected, then R^2 of *FFO* exceeds that of *NI*. Stated differently, testing for the R^2 difference between *FFO* and *NI* is a special case of parameter restrictions.[9]

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[8] Results are comparable when we use raw return, abnormal return adjusted by equally weighted index, and abnormal return adjusted by size deciles.

[9] Details of this proof are available upon request.

TABLE 1
Variable Definitions

Variable (firm subscript omitted)		**Description (Compustat data item numbers in brackets)**
$ACCUDEPps_t$	=	Accumulated depreciation and amortization per share adjusted for stock splits: $390_t/(25 * 27)_t$ for REITs; $196_t/(25 * 27)_t$ for non-REITs.
$ACCUDEP\_SOLDps_t$		Accumulated depreciation and amortization of properties sold per share (accumulated depreciation and amortization at the beginning of year t, plus depreciation expense for year t, minus accumulated depreciation and amortization at the end of year t, scaled by shares outstanding at end of year t adjusted for stock split): $(390_{t-1} + 393_t - 390_t)/(25_t * 27_t)$ for REITs and: $(196_{t-1} + 14_t - 196_t)/(25_t * 27_t)$ for non-REITs.
AR_t	=	Abnormal return measured from the beginning of the fourth month of the current fiscal year until the end of the third month of the following fiscal year, computed as raw return minus return on the value weighted CRSP Index.
BM_t	=	Book-to-market ratio at the beginning of year t: $(60_{t-1}/(199 * 25)_{t-1})$.
$BVEps_t$	=	Book value of equity per share adjusted for stock splits: $60_t/(25 * 27)_t$.
DEP_t	=	Depreciation and amortization expense per share scaled by stock price at the beginning of year t: $393_t/(25 * 199)_{t-1}$ for REITs; $14_t/(25 * 199)_{t-1}$ for non-REITs.
ΔDEP_t	=	Change in depreciation and amortization expense per share for year t scaled by beginning-of-the-year stock price: $(393_t - 393_{t-1})/(25 * 199)_{t-1}$ for REITs; $(14_t - 14_{t-1})/(25 * 199)_{t-1}$ for non-REITs.
$DIVps_t$	=	Dividend per share adjusted for stock splits: $21_t/(25 * 27)_t$.
FFO_t	=	Fund from operations per share scaled by beginning-of-the-year stock price: $(18 + 393 - 392)_t/(25 * 199)_{t-1}$ for REITs; $(18 + 14 - 365)_t/(25 * 199)_{t-1}$ for non-REITs.
ΔFFO_t	=	Change in FFO per share for year t scaled by beginning-of-the-year stock price: $((18 + 393 - 392)_t - (18 + 393 - 392)_{t-1})/(25 * 199)_{t-1}$ for REITs; $((18 + 14 - 365)_t - (18 + 14 - 365)_{t-1})/(25 * 199)_{t-1}$ for non-REITs.
$GAIN_t$	=	Gain (or loss) per share scaled by stock price at the beginning of year t: $392_t/(25 * 199)_{t-1}$ for REITs; $365_t/(25 * 199)_{t-1}$ for non-REITs.
$\Delta GAIN_t$	=	Change in gain (or loss) per share from year t–1 to year t scaled by stock price at the beginning of year t: $(392_t - 392_{t-1})/(25 * 199)_{t-1}$ for REITs; $(365_t - 365_{t-1})/(25 * 199)_{t-1}$ for non-REITs.
$GAINps_t$	=	Gain (or loss) per share adjusted for stock split: $392_t/(25 * 27)_t$ for REITs; $365_t/(25 * 27)_t$ for non-REITs.
$LANDps_t$	=	Land per share adjusted for stock split. For REITs *LANDps* = hand-collected land value$/(25 * 27)_t$; for non-REITs, *LANDps* = $260_t/(25 * 27)_t$.
LEV_t	=	Leverage ratio: $(6 - 60)_t/6_t$.
$\lg MV_t$	=	Natural log of market capitalization at the beginning of year t: $\ln(199 * 25)_{t-1}$.

(*continued on next page*)

TABLE 1 (continued)

Variable (firm subscript omitted)		Description (Compustat data item numbers in brackets)
NI_t	=	GAAP net income per share scaled by the beginning stock price: $18_t/(25 * 199)_{t-1}$.
ΔNI_t	=	Change in GAAP net income per share from year $t-1$ to year t scaled by stock price at the beginning of year t: $(18_t - 18_{t-1})/(25 * 199)_{t-1}$.
$NIps_t$	=	GAAP net income per share adjusted for stock splits: $18_t/(25 * 27)_t$.
$NOAps_t$	=	Net operating asset per share adjusted for stock splits: $(6 - 1 - 9 - 34)_t/(25 * 27)_t$.
P_t	=	Stock price per share at the annual earnings announcement date, adjusted for stock splits.
$SALEps_t$	=	Sales per share adjusted for stock split: $12_t/(25 * 27)_t$.
SGR_t	=	Sale growth from year $t-1$ to year t: $(12_t/12_{t-1}) - 1$.

To evaluate H1b, we test for the equality of coefficients of the variables of interest (ΔFFO, ΔDEP, and $\Delta GAIN$) between the REITs and non-REITs using stacked data. Specifically, we allow each group (REIT and non-REIT) to have separate intercepts and slope coefficients in a pooled regression. We then test whether (1) $\alpha_{1,\mathrm{REIT}} = \alpha_{1,\mathrm{non\text{-}REIT}}$; (2) $\alpha_{2,\mathrm{REIT}} = \alpha_{2,\mathrm{non\text{-}REIT}}$; (3) $\alpha_{3,\mathrm{REIT}} = \alpha_{3,\mathrm{non\text{-}REIT}}$; and (4) $\alpha_{1,\mathrm{REIT}} = \alpha_{1,\mathrm{non\text{-}REIT}}$, $\alpha_{2,\mathrm{REIT}} = \alpha_{2,\mathrm{non\text{-}REIT}}$, $\alpha_{3,\mathrm{REIT}} = \alpha_{3,\mathrm{non\text{-}REIT}}$, where the last is a joint test of a set of coefficient restrictions across two groups.

Hypotheses on Accumulated Depreciation

If accounting depreciation is an appropriate proxy for obsolescence and impairment of fixed assets, then the book value of assets approximates market value, and thus, accumulated depreciation would be unrelated to stock price. On the other hand, if the alleged irrelevance of depreciation expense is shared by the market, then stock prices reflect the understatement of owners' equity attributable to over-depreciation. As a result, stock price and accumulated depreciation would be positively associated. Hypotheses 2a and 2b, stated in the null, test for such a relationship:

H2a: There is no association between stock price and accumulated depreciation.

H2b: The association between stock price and accumulated depreciation does not differ between REIT and non-REIT firms.

With respect to H2a, rejecting the null hypothesis in favor of a positive association supports the *relevance* of accumulated depreciation after considering GAAP net income and book value of equity (net book value of assets).[10] To test these hypotheses, we adopt the Ohlson (1995) model consistent with previous value relevance research (Barth and McNichols 1994; Fields et al. 1998; Francis and Schipper 1999). Prior applications of the Ohlson model typically include *NI*, the book value of equity, and dividends as independent variables (Aboody and Lev 1998; Barth et al. 1998; Barth and Clinch 1998), along with additional variables on an *ad hoc* basis. We augment the Ohlson model with the accumulated depreciation and also include sales growth and leverage as control variables (Hann et al. 2007; Penman et al. 2007). The following specification considers H2a:

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[10] Notice that value *relevance* of accumulated depreciation is conditional on the presence of GAAP net income and book value of equity. An equivalent statement is that "Accumulated depreciation is value-*irrelevant* in the presence of FFO and book value of equity before deducting accumulated depreciation."

$$P_{i,t} = \beta_0 + \beta_1 NIps_{i,t} + \beta_2 BVEps_{i,t} + \beta_3 ACCUDEPps_{i,t} + \beta_4 DIVps_{i,t} + \beta_5 SGR_{i,t} + \beta_6 LEV_{i,t} + \sum_{i=2000}^{2004} \beta_i Y_i + \xi_{i,t}, \tag{2}$$

where P is stock price at the annual earnings announcement date; $NIps$ is GAAP NI before extraordinary items; $BVEps$ is the book value of stockholders' equity; $ACCUDEPps$ is accumulated depreciation and amortization; $DIVps$ is dividend; and SGR and LEV are sales growth and financial leverage, respectively. All variables are expressed on a per-share basis, with the exception of SGR, LEV, and the year indicator variables. We note that Fields et al. (1998) use a levels specification to examine the relative explanatory power between FFO and NI. Model (2) differs from their model because we focus on value relevance of accumulated depreciation, a factor that Fields et al. (1998) do not consider.

Barth and Kallapur (1996) point out that coefficient estimates in a cross-sectional level-based regression are potentially biased due to scale differences. To mitigate this problem, we estimate model (2) using two alternative approaches. First, we use weighted least-squares combined with Huber-White robust standard error correction (Greene 2003, 227). Second, we scale all variables, except SGR, LEV, and year indicators, with lagged assets per share and estimate model (2) using ordinary least squares (OLS) (Ball and Shivakumar 2005). We do not scale variables with lagged price (Brown et al. 1999) or include lagged price as an additional independent variable (Barth and Kallapur 1996) because lagged price also captures the value relevance of accumulated depreciation.[11]

If accumulated depreciation is not value-relevant (in the presence of GAAP net income and book value of equity), then we anticipate $\beta_3 = 0$. Evidence that $\beta_3 > 0$ supports a characterization that investors perceive that properties are over-depreciated.

The levels model offers the following advantages. First, it does not require an expectation model, as in specification (1). Second, the balance sheet at any given time captures the cumulative impact of all past understatements of assets, whereas the returns model captures only a one-year effect. Thus, the levels model focusing on accumulated depreciation offers a powerful test of the claim that taking depreciation on the REIT properties understates firm performance and value. On the other hand, the levels specification can be subject to the omitted variables problem, as all variables that potentially affect stock prices cannot be included. Finally, we evaluate H2b by testing for coefficient equality between REITs and non-REITs.

Hypotheses Based on Non-Stock Market Data

Hypothesis on the Relationship between Depreciation and Gain (or Loss) from Sales

It is well known that accounting depreciation is unequal to economic depreciation (Hotelling 1925; Beaver 1998) and that accounting rate of return differs from economic rate of return (Fisher and McGowan 1983; Beaver 1998, Chapters 3 and 4). Under Hotelling's (economic) depreciation, an asset's book value is equal to the present value of the future cash flows that the asset can generate (Beaver 1998). In such a case, economic depreciation is the change in market value of the asset. Under economic depreciation, therefore, no gain or loss arises from the sale of an asset. In practice, accounting depreciation only coincidentally equals Hotelling depreciation, and thus accounting gains or losses arise. Focusing on this point, we develop a hypothesis and test procedures that allow us to examine the extent to which accounting depreciation departs from economic depreciation for REITs and non-REITs.

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[11] We thank anonymous referees for this point.

Denote α_E and α_A as a firm's economic and accounting *accumulated* depreciation, respectively, expressed as a fraction of the acquisition cost (AC), where $0 < \alpha_E \leq 1$ and $0 < \alpha_A \leq 1$. In an ideal world of economic depreciation, the fair market value (MV) of an asset equals the acquisition cost less accumulated *economic* depreciation (α_E). Thus:

$$MV = AC(1 - \alpha_E). \tag{3}$$

Given a competitive market for real estate assets, the selling value (SV) of the property equals the fair market value in the sense that $SV = MV + \varepsilon$, where $E(\varepsilon) = 0$. To the extent that accounting accumulated depreciation deviates from economic accumulated depreciation, however, an accounting gain ($AGAIN$) arises from the asset sale:

$$\begin{aligned} AGAIN &= SV - BV = (MV + \varepsilon) - AC(1 - \alpha_A) = AC(1 - \alpha_E) \\ &\quad + \varepsilon - AC(1 - \alpha_A) = AC(\alpha_A - \alpha_E) + \varepsilon, \end{aligned} \tag{4}$$

where BV is the asset's book value, or $AC(1 - \alpha_A)$. To convert Expression (4) into a testable relation between depreciation and accounting gain, designate the dollar amount of accounting *accumulated* depreciation at the time of the sale as DEP_A. Then, $DEP_A = AC * \alpha_A$. Alternatively:

$$AC = DEP_A / \alpha_A. \tag{5}$$

Expressions (4) and (5) jointly imply that:

$$AGAIN = \left(1 - \frac{\alpha_E}{\alpha_A}\right) DEP_A + \varepsilon. \tag{6}$$

Expression (6) establishes a testable relation between accounting gain ($AGAIN$) and accumulated depreciation (DEP_A). When accounting accumulated depreciation equals economic accumulated depreciation ($\alpha_A = \alpha_E$), the parameter $(1 - (\alpha_E/\alpha_A))$ is zero; that is, there is no association between gain from sale ($AGAIN$) and accounting accumulated depreciation (DEP_A). The association is nonzero, however, when accounting depreciation consistently misestimates economic depreciation. More specifically, when accounting depreciation exceeds economic depreciation ($\alpha_A > \alpha_E$), then $0 < (1 - (\alpha_E/\alpha_A)) < 1$, and gain from sale is *positively* related to accounting accumulated depreciation. Conversely, when accounting depreciation is less than economic depreciation ($\alpha_A < \alpha_E$), then $(1 - (\alpha_E/\alpha_A)) < 0$, and gain from sale is *negatively* related to accounting accumulated depreciation. The foregoing reasoning suggests the following hypotheses:

H3a: There is no association between gain (or loss) from sales and accumulated depreciation of sold properties.

H3b: The association between gain (or loss) from sales and accumulated depreciation of sold properties does not differ between REIT and non-REIT firms.

We address H3a using the following specification:

$$GAINps_{i,t} = \lambda_0 + \lambda_1 ACCUDEP\_SOLDps_{i,t} + \upsilon_{i,t}, \tag{7}$$

where *GAINps* is gain (or loss) from sale of assets per share and *ACCUDEP_SOLDps* is accumulated depreciation of properties sold per share. Rejecting $\lambda_1 = 0$ in favor of $\lambda_1 > 0$ ($\lambda_1 < 0$) suggests that accounting depreciation is greater (less) than economic depreciation and net book value is understated (overstated).

With respect to H3b, notice that α_E/α_A decreases as the extent of over-depreciation increases, and therefore, the parameter estimate λ_1 increases. Hence, $\lambda_{1REIT} > \lambda_{1Non\text{-}REIT}$ supports a conclusion that REITs' properties are over-depreciated relative to non-REITs' properties.

The Ability of Accumulated Depreciation to Predict Future Benefits

Under GAAP, long-term properties are recorded as assets because they are expected to generate future benefits. If reported accumulated depreciation properly represents the economically depreciated, unproductive, portion of fixed assets, then it is unrelated to future benefits. If accounting depreciation exceeds economic depreciation, however, then the book value of fixed assets understates productive assets and accumulated depreciation would be related to future benefits. The null hypotheses follow:

H4a: There is no association between future revenues and accumulated depreciation.

H4b: The association between future revenue and accumulated depreciation does not differ between REIT and non-REIT firms.

The following model tests for the predictive content of accumulated depreciation:

$$SALEps_{i,t} = \gamma_0 + \gamma_1 NOAps_{i,t-1} + \gamma_2 ACCUDEPps_{i,t-1} + \omega_{i,t}, \tag{8}$$

where $SALESps_t$ is net sales per share at year *t* and $NOAps_{t-1}$ and $ACCUDEP_{t-1}$ are lagged net operating assets per share and total accumulated depreciation per share, respectively. Model (8) posits that greater current net operating assets (*NOA*) produce greater future revenues in the sense of asset turnover.[12] Finding $\gamma_2 = 0$ indicates that accumulated depreciation does not have incremental explanatory power above and beyond *NOA*. Finding $\gamma_2 > 0$ supports the relevance of accumulated depreciation in predicting future benefits (revenues).

V. SAMPLE

We obtain both REIT (SIC code 6798) and non-REIT financial data from the Compustat industrial annual file, and stock return information from the CRSP file. The sample period covers 2000 through 2005. The sample period begins in 2000 because, as discussed in Section II, the new definition of FFO is effective in 2000. The Compustat database reports real-estate-related line items beginning in 2001 (data 390 through data 393). To ensure that we cover all periods during which the new FFO definition applies, we hand-collect additional data for 1999 and 2000 from annual reports and 10-K filings. As we use 1999 as the base year, the sample period covers 2000–2005.

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[12] We do not test whether accumulated depreciation predicts NI or FFO, because the core controversy is precisely whether NI or FFO is the appropriate metric for performance.

The initial REIT (non-REIT) sample has 1,146 (65,429) firm-year observations for 191 (9,347) firms. We exclude observations with missing values, and remove observations with values beyond the top and bottom 1 percentile. This process yields 468 firm-year observations for 103 REIT firms and 14,188 firm-year observations for 3,541 non-REIT firms.

REIT is the most capital-intensive industry, with the highest fixed asset ratio (PPE/noncash total assets) of all industries. Untabulated results show that the mean fixed asset ratio for the REIT sample is 0.87, compared with 0.31 for the non-REIT sample.[13] To ensure that results are not driven by differences in fixed asset ratio, we construct a matching sample of non-REIT firms, as follows. For each REIT observation, we select a non-REIT observation (from the 14,188 observations) that (1) has the same fiscal year and a fixed asset ratio closest to that of the REIT observation, and (2) is from an industry with mean industry fixed asset ratio of 0.5 or greater, based on the Fama and French (1997) industry classification.

Tables 2 and 3 provide descriptive statistics and Pearson correlations for independent variables used in regression specifications for both REITs (lower diagonal) and non-REITs (upper diagonal), respectively. Descriptive statistics for the REIT group are comparable to those of prior studies.[14] The mean fixed asset ratio (*FAratio*) for REITs is slightly higher than the *FAratio* for capital-intensive non-REIT firms (0.87 versus 0.86, $p = 0.082$, two-tailed) because no other industry is as capital-intensive as the REIT industry.

If properties held by REITs are primarily for investment purposes, then we anticipate that (1) REITs report a greater frequency of property sales; (2) REIT properties are less subject to obsolescence and impairment and, thus, REITs report a greater frequency of gains than losses; and (3) REITs' gains from sales (operating income) account for a higher (lower) proportion of total income *vis-à-vis* non-REIT firms. Untabulated results indicate that the frequency of property sale for REITs (0.55; 258 of 468 firm years) is higher than the frequency for non-REITs (0.09; 43 of 468 firm years), with a significance level of less than 1 percent. Furthermore, 227, or 88 percent, of REITs' property sales report gains, compared with 33, or 76.7 percent, of non-REITs' property sales (p-value for the difference = 0.047). Finally, the average gain as a fraction of total *NI* is 9.08 percent for REITs, compared with 0.36 percent for non-REITs (p-value for the difference < 0.001). In sum, these preliminary statistics are consistent with the characterization that REITs' properties are sold more often than non-REITs' properties, as is expected for properties held for investment.

RESULTS

Information Content of Depreciation Expense

Panel A of Table 4 contains estimates for the returns specification (1). The first and second columns display estimates for non-REITs and REITs, respectively. The third column reports statistics for testing whether each of the coefficients is equal between REITs and

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[13] The fixed asset ratios in this study are consistent with the fixed asset ratios of Barclay and Smith (2005), who report that the average fixed asset ratio for REITs was 0.8, compared with 0.3 for industrial firms, during 1970–2005.

[14] Prior studies do not use non-REIT firms as a comparison group and thus do not report corresponding descriptive statistics.

TABLE 2
Descriptive Statistics

Variables	n	Mean	Median	Std. Dev.	1st Quartile	3rd Quartile
Non-REIT Firms						
ACCUDEPps	468	**7.627**	5.189	7.864	2.176	10.406
ACCUDEP_SOLDps	468	**0.236**	**0.072**	1.242	0.003	0.309
AR	468	0.291	0.178	0.576	−0.049	0.527
BM	468	0.581	**0.493**	0.399	0.349	0.690
BVEps	468	**8.767**	**7.051**	6.733	3.856	12.231
DEP	468	**0.087**	**0.063**	0.098	0.028	0.108
ΔDEP	468	**0.010**	**0.005**	0.019	0.001	0.014
DIVps	468	**0.219**	**0.000**	0.412	0.000	0.242
FAratio	468	0.855	**0.894**	0.127	0.838	0.920
FFO	468	**0.136**	0.114	0.162	0.054	0.193
ΔFFO	468	**0.021**	**0.012**	0.085	−0.006	0.041
GAIN	468	**0.000**	**0.000**	0.002	0.000	0.000
ΔGAIN	468	0.000	0.000	0.002	0.000	0.000
GAINps	468	**0.004**	**0.000**	0.050	0.000	0.000
LANDps	141	**2.263**	**1.112**	2.980	0.037	3.556
LEV	468	**0.520**	**0.540**	0.201	0.391	0.660
lg*MV*	468	**6.188**	**6.122**	1.904	4.695	7.547
NI	468	0.049	0.051	0.100	0.016	0.084
ΔNI	468	**0.011**	**0.006**	0.080	−0.010	0.028
NIps	468	**0.837**	**0.707**	1.099	0.182	1.427
NOAps	468	**13.605**	**10.140**	12.633	5.057	19.270
P	468	**17.947**	**16.195**	13.225	7.407	25.010
SALEps	468	**11.899**	**7.154**	17.117	2.601	15.591
SGR	468	**0.242**	**0.113**	0.538	0.008	0.341
REIT Firms						
ACCUDEPps	468	**5.954**	5.176	3.612	3.330	8.176
ACCUDEP_SOLDps	468	**0.366**	**0.230**	0.524	0.075	0.477
AR	468	0.245	0.234	0.242	0.081	0.387
BM	468	0.623	**0.591**	0.286	0.445	0.762
BVEps	468	**14.085**	**13.338**	6.644	8.722	19.481
DEP	468	**0.062**	**0.050**	0.048	0.033	0.074
ΔDEP	468	**0.006**	**0.004**	0.010	0.001	0.009
DIVps	468	**1.496**	**1.502**	0.748	0.941	1.965
FAratio	468	0.870	**0.914**	0.144	0.838	0.950
FFO	468	**0.112**	0.098	0.069	0.077	0.133
ΔFFO	468	**0.008**	**0.007**	0.029	−0.001	0.018
GAIN	468	**0.008**	**0.000**	0.024	0.000	0.005
ΔGAIN	468	0.001	0.000	0.024	−0.002	0.001
GAINps	468	**0.148**	**0.000**	0.377	0.000	0.106
LANDps	468	**7.304**	**6.975**	4.482	3.336	10.115
LEV	468	**0.641**	**0.638**	0.149	0.564	0.743
lg*MV*	468	**6.542**	**6.707**	1.426	5.566	7.545
NI	468	0.057	0.053	0.051	0.032	0.075
ΔNI	468	**0.003**	**0.001**	0.034	−0.008	0.013
NIps	468	**1.244**	**1.174**	0.872	0.737	1.807
NOAps	468	**19.954**	**18.068**	10.148	12.136	26.546
P	468	**26.471**	**25.200**	14.530	14.845	34.100
SALEps	468	**6.622**	**6.165**	3.575	3.570	8.806
SGR	468	**0.149**	**0.091**	0.297	0.026	0.185

(*continued on next page*)

TABLE 2 (continued)

Bold text indicates significant differences between the non-REIT sample and REIT sample at the 5 percent level or better, two-tailed. Differences in means (medians) are assessed using a t-test (Wilcoxon rank sum test).

Variable Definitions:
ACCUDEPps = accumulated depreciation per share;
ACCUDEP_SOLDps = accumulated depreciation for sold properties per share;
AR = abnormal stock return;
BM = book-to-market ratio;
BVEps = book value of equity per share;
DEP = depreciation and amortization per share scaled by stock price at the beginning of the year;
ΔDEP = change in depreciation and amortization per share scaled by stock price at the beginning of the year;
DIVps = dividend per share;
FAratio = fixed asset ratio (net PPE/total assets other than cash);
FFO = *FFO* per share scaled by stock price at the beginning of the year;
ΔFFO = change in *FFO* per share scaled by stock price at the beginning of the year;
GAIN = gain (or loss) from sales per share scaled by stock price at the beginning of the year;
$\Delta GAIN$ = change in gain (or loss) from sales per share scaled by stock price at the beginning of the year;
GAINps = gain (or loss) from sales per share;
LANDps = land per share;
LEV = leverage ratio;
lg*MV* = log market value of equity at the beginning of the year;
NI = net income per share scaled by stock price at the beginning of the year;
ΔNI = change in net income per share scaled by stock price at the beginning of the year;
NIps = net income per share;
NOAps = net operating asset per share;
P = stock price at annual earnings announcement date;
SALEps = sales per share; and
SGR = sales growth rate.

See Table 1 for precise definitions of the variables.

non-REITs, without restricting all remaining coefficients to be identical. Results are comparable when we assume equality of all remaining coefficients. F-test statistics for coefficient equality of a *group* of variables are reported in the last three rows.

As expected, ΔFFO is positively and significantly related to stock returns for both groups ($p < 0.001$). There is little evidence that ΔDEP is significantly related to abnormal returns, however, for either REITs or non-REITs, where the associated p-values are 0.638 and 0.435, respectively. Judging from the results in Panel A of Table 4, the null hypothesis of no significant association between stock returns and depreciation (H1a) cannot be rejected at the conventional significance level for either the non-REIT or the REIT sample. Similarly, there is little evidence that $\Delta GAIN$ is significantly associated with stock returns. This is consistent with Fields et al. (1998), who report a similar lack of association for REITs, and with Barth et al. (1990), who find that realized gains and losses on securities held by banks are not significantly related to stock returns.

REIT and non-REIT samples differ, however, regarding parameter restrictions across ΔFFO, ΔDEP, and $\Delta GAIN$. More specifically, the parameter restriction $\alpha_1 = -\alpha_2 = \alpha_3$ is rejected for the REIT group ($p = 0.003$), but not for the non-REIT group ($p = 0.344$). Thus, evidence that parameter estimates for ΔDEP and $\Delta GAIN$ differ from parameter estimates for ΔFFO is stronger for the REIT sample, supporting the notion that NI is *not* a superior summary indicator of performance for REITs. Nevertheless, pair-wise tests of coefficient difference (column 3) indicate that estimates for ΔFFO, ΔDEP, and $\Delta GAIN$ are not significantly different between REITs and non-REITs.

TABLE 3
Pearson Correlations for Non-REIT (upper diagonal) and REIT (lower diagonal) Firms (n = 468 for non-REIT and REIT firms, respectively)

	Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
(1)	*AR*	1	0.390***	0.114**	0.013	−0.040	0.096**	0.146***	0.102**	−0.098**
(2)	Δ*FFO*	0.119**	1	0.366***	0.013	−0.049	0.122***	0.051	0.307***	0.006
(3)	Δ*DEP*	0.078*	0.279***	1	−0.043	0.006	−0.113**	0.075	0.170***	0.109**
(4)	Δ*GAIN*	0.053	−0.109**	0.080*	1	0.064	−0.002	0.032	0.064	0.110**
(5)	lg*MV*	0.035	−0.074	−0.028	−0.052	1	−0.336***	0.487***	0.397***	0.370***
(6)	*BM*	0.004	0.024	−0.046	0.008	−0.373***	1	−0.221***	−0.120***	0.144***
(7)	*P*	0.223***	0.005	0.051	−0.040	0.530***	−0.337***	1	0.637***	0.696***
(8)	*NIps*	0.090*	0.090*	0.011	0.145***	0.306***	−0.129***	0.531***	1	0.583***
(9)	*BVEps*	0.032	−0.029	0.010	−0.036	0.403***	0.148***	0.597***	0.570***	1
(10)	*ACCUDEPps*	0.028	−0.066	−0.001	0.005	0.252***	−0.205***	0.499***	0.112**	0.220***
(11)	*DIVps*	0.026	−0.114**	−0.013	−0.011	0.393***	−0.090*	0.522***	0.580***	0.671***
(12)	*SGR*	0.064	0.158***	0.380***	0.014	−0.091**	0.091**	−0.043	−0.132***	−0.040
(13)	*LEV*	0.005	0.049	0.253**	0.074	0.014	−0.277***	0.099**	−0.109**	−0.266***
(14)	*GAINps*	0.037	−0.113**	0.054	0.606***	0.034	0.001	0.076*	0.347***	0.174***
(15)	*ACCUDEP_SOLDps*	−0.015	−0.176***	−0.119***	0.165***	0.121***	−0.066	0.174***	−0.048	0.219***
(16)	*SALEps*	0.016	0.039	0.149***	0.032	0.271***	−0.088*	0.511***	0.310***	0.439***
(17)	*NOAps*	0.008	0.009	0.107**	−0.008	0.372***	0.041	0.598***	0.586***	0.823***

(*continued on next page*)

TABLE 3 (continued)

	Variables	(10)	(11)	(12)	(13)	(14)	(15)	(16)	(17)
(1)	*AR*	−0.126***	−0.105**	0.238***	−0.056	−0.012	0.030	−0.045	−0.067
(2)	*ΔFFO*	0.021	−0.092**	0.249***	−0.022	−0.024	−0.018	−0.036	−0.026
(3)	*ΔDEP*	0.186***	−0.099**	0.278***	0.162***	−0.078*	−0.019	0.050	0.095**
(4)	*ΔGAIN*	−0.039	−0.010	−0.022	−0.044	0.814***	0.030	0.030	0.051
(5)	lg*MV*	0.292***	0.096**	0.015	0.156***	0.104**	0.059	0.186***	0.379***
(6)	*BM*	0.054	−0.056	−0.151***	−0.127***	0.027	0.106**	0.025	0.088*
(7)	*P*	0.467***	0.299***	0.029	0.076	0.044	0.080*	0.397***	0.643***
(8)	*NIps*	0.397***	0.343***	0.030	0.057	0.060	0.207***	0.265***	0.531***
(9)	*BVEps*	0.682***	0.347***	−0.129***	0.099**	0.159***	0.197***	0.499***	0.884***
(10)	*ACCUDEPps*	1	0.304***	−0.114**	0.315***	−0.021	0.079*	0.430***	0.723***
(11)	*DIVps*	0.279***	1	−0.155***	0.207***	−0.008	0.083*	0.291***	0.411***
(12)	*SGR*	−0.186***	−0.165***	1	0.009	−0.042	−0.082*	−0.059	−0.126***
(13)	*LEV*	0.471***	−0.123***	0.009	1	−0.043	0.124***	0.199***	0.292***
(14)	*GAINps*	0.115**	0.173***	−0.067	0.116**	1	0.041	0.027	0.077*
(15)	*ACCUDEP_SOLDps*	0.318***	0.181***	−0.207***	0.163***	0.415***	1	0.251***	0.311***
(16)	*SALEps*	0.750***	0.331***	0.042	0.503***	0.229***	0.258***	1	0.551***
(17)	*NOAps*	0.402***	0.578***	−0.092**	0.082*	0.200***	0.204***	0.635***	1

(*continued on next page*)

TABLE 3 (continued)

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*, **, *** Denote two-tailed significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

Variable Definitions:

AR = abnormal stock return;
ΔFFO = change in *FFO* per share scaled by stock price at the beginning of the year;
ΔDEP = change in depreciation and amortization per share scaled by stock price at the beginning of the year;
$\Delta GAIN$ = change in gain (or loss) from sales per share scaled by stock price at the beginning of the year;
lgMV = log market value of equity at the beginning of the year;
BM = book-to-market ratio;
P = stock price at annual earnings announcement date;
NIps = net income per share;
BVEps = book value of equity per share;
ACCUDEPps = accumulated depreciation per share;
DIVps = dividend per share;
SGR = sales growth rate;
LEV = leverage ratio;
GAINps = gain (or loss) from sales per share;
ACCUDEP_SOLDps = accumulated depreciation for sold properties per share;
SALEps = sales per share;
NOAps = net operating asset per share; and

See Table 1 for precise definitions.

TABLE 4
Test of Information Content of Depreciation Expense Using Stock Returns

Panel A: Stock Return Model Using Change Variables

Model: $AR_{i,t} = \alpha_0 + \alpha_1 \Delta FFO_{i,t} + \alpha_2 \Delta DEP_{i,t} + \alpha_3 \Delta GAIN_{i,t} + \alpha_4 \mathrm{lg}MV_{i,t-1} + \alpha_5 BM_{i,t-1} + \sum_{i=2000}^{2004} \alpha_i Y_i + \varepsilon_{i,t}$

	Non-REITs			REITs			Tests of Coefficient Differences[a]	
	Coefficient	**t-stat.**	**p-value**	**Coefficient**	**t-stat.**	**p-value**	**t-stat.**	**p-value**
Intercept	0.331	2.889	0.004***	0.207	3.758	0.000***	0.976	0.330
ΔFFO	2.154	5.570	0.000***	1.756	4.022	0.000***	0.680	0.495
ΔDEP	−0.989	−0.782	0.435	−0.411	−0.471	0.638	−0.380	0.707
$\Delta GAIN$	−1.275	−0.347	0.729	0.091	0.251	0.802	−0.370	0.712
lg*MV*	−0.003	−0.211	0.833	0.014	2.296	0.024**	−1.150	0.250
BM	0.097	1.487	0.138	0.008	0.216	0.829	1.180	0.239
Y2000	−0.130	−1.613	0.108	−0.010	−0.308	0.759	−1.390	0.165
Y2001	−0.211	−2.737	0.007***	−0.021	−0.689	0.492	−2.280	0.023**
Y2002	−0.494	−7.577	0.000***	−0.306	−9.929	0.000***	−2.620	0.093*
Y2003	0.141	1.719	0.087*	0.206	6.685	0.000***	−0.740	0.458
Y2004	−0.001	−0.020	0.984	−0.244	−10.733	0.000***	3.140	0.002***
n		468			468			
R^2		0.272			0.519			
Test of Coefficient Restrictions (p-values)								
$\alpha_2 = \alpha_3 = 0$			0.717			0.888		
$\alpha_1 = -\alpha_2 = \alpha_3$			0.344			0.003***		
$\alpha_{1\_nonREIT} = \alpha_{1\_REIT}, \alpha_{2\_nonREIT} = \alpha_{2\_REIT}, \alpha_{3\_nonREIT} = \alpha_{3\_REIT}$								0.898

(continued on next page)

TABLE 4 (continued)

Panel B: Stock Return Model Using Level and Change Variables

Model: $AR_{i,t} = \alpha_0 + \alpha_1 FFO_{i,t} + \alpha_2 \Delta FFO_{i,t} + \alpha_3 DEP_{i,t} + \alpha_4 \Delta DEP_{i,t} + \alpha_5 GAIN_{i,t} + \alpha_6 \Delta GAIN_{i,t} + \alpha_7 \mathrm{lg}MV_{i,t-1} + \alpha_8 BM_{i,t-1} + \sum_{i=2000}^{2004} \alpha_i Y_i + \varepsilon_{i,t}$

	Non-REITs			REITs			Tests of Coefficient Differences[a]	
	Coefficient	**t-stat.**	**p-value**	**Coefficient**	**t-stat.**	**p-value**	**t-stat.**	**p-value**
Intercept	0.336	3.040	0.002***	0.183	3.438	0.001***	1.220	0.223
FFO	0.783	2.410	0.016**	0.399	1.814	0.073*	1.000	0.316
ΔFFO	1.873	4.357	0.000***	1.592	3.678	0.000***	0.470	0.637
DEP	−0.730	−1.472	0.142	−0.151	−0.546	0.587	−1.040	0.297
ΔDEP	−2.443	−1.774	0.077*	−0.752	−0.889	0.376	−1.070	0.286
GAIN	−8.414	−0.801	0.424	−0.114	−0.160	0.873	−0.860	0.390
ΔGAIN	3.997	0.423	0.673	0.211	0.397	0.692	0.430	0.674
lg*MV*	−0.006	−0.466	0.642	0.015	2.632	0.010***	−1.500	0.133
BM	0.102	1.424	0.155	−0.002	−0.062	0.951	1.320	0.188
Y2000	−0.169	−2.104	0.036**	−0.038	−1.127	0.262	−1.470	0.144
Y2001	−0.214	−2.643	0.008***	−0.041	−1.278	0.204	−2.110	0.036**
Y2002	−0.498	−7.562	0.000***	−0.319	−10.313	0.000***	−2.520	0.012**
Y2003	0.129	1.510	0.132	0.196	6.386	0.000***	−0.750	0.455
Y2004	−0.005	−0.066	0.947	−0.246	−10.805	0.000***	3.160	0.002***
n		468			468			
R^2		0.291			0.525			
Test of Coefficient Restrictions (p-values)								
$\alpha_3 + \alpha_4 = 0$			0.023**			0.260		
$\alpha_5 + \alpha_6 = 0$			0.163			0.838		
$\alpha_3 + \alpha_4 = \alpha_5 + \alpha_6 = 0$			0.057*			0.527		
$(\alpha_1 + \alpha_2) = -(\alpha_3 + \alpha_4) = (\alpha_5 + \alpha_6)$			0.085*			0.008***		
Joint restriction: $(\alpha_1 + \alpha_2)$_nonREIT = $(\alpha_1 + \alpha_2)$_REIT, $(\alpha_3 + \alpha_4)$_nonREIT = $(\alpha_3 + \alpha_4)$_REIT, $(\alpha_5 + \alpha_6)$_nonREIT = $(\alpha_5 + \alpha_6)$_REIT								0.3233

(*continued on next page*)

TABLE 4 (continued)

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*, **, *** Denote two-tailed significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

[a] Tests of coefficient differences are based on a stacked regression where both REIT and non-REIT observations are pooled and each group has separate intercept and slope coefficients.

Reported estimates are from pooled regressions over 2000–2005.

Standard errors are based on Huber-White corrections clustered by firm.

Variable Definitions:

AR = abnormal return calculated as raw returns minus return on the value weighted CRSP Index;
Δ*FFO* = change in *FFO* per share from year $t-1$ to year t scaled by stock price at the beginning of fiscal year;
Δ*DEP* = change in depreciation and amortization expense per share from year $t-1$ to year t scaled by stock price at the beginning of fiscal year;
Δ*GAIN* = change in gain or loss per share from year $t-1$ to year t scaled by stock price at the beginning of fiscal year;
FFO_t = *FFO* per share scaled by stock price at the beginning of fiscal year;
DEP = depreciation and amortization expense per share scaled by stock price at the beginning of fiscal year;
$GAIN_t$ = gain (or loss) per share scaled by stock price at the beginning of fiscal year;
lg*MV* = natural log of market capitalization at the beginning of fiscal year;
BM = book-to-market ratio at the beginning of fiscal year; and
Y2000–Y2004 = indicator variables that equal 1 for the corresponding fiscal year, and 0 otherwise.

See Table 1 for precise definitions of the variables.

Given the mixed evidence using the returns model, we also estimate a model using both levels *and* changes of earnings, following Vincent (1999) and Biddle et al. (1995):

$$AR_{i,t} = \alpha_0 + \alpha_1 FFO_{i,t} + \alpha_2 \Delta FFO_{i,t} + \alpha_3 DEP_{i,t} + \alpha_4 \Delta DEP_{i,t} + \alpha_5 GAIN_{i,t} + \alpha_6 \Delta GAIN_{i,t} + \alpha_7 \lg MV_{i,t-1} + \alpha_8 BM_{i,t-1} + \sum_{i=2000}^{2004} \alpha_i Y_i + \varepsilon_{i,t}. \tag{9}$$

Ali and Zarowin (1992) and Biddle et al. (1995) show that the use of both levels and changes of earnings as proxies for unexpected earnings reduces measurement error in unexpected earnings when earnings have transitory components. In Equation (9), the ERC is the sum of the coefficients on the level and change variables (Brown et al. 1987).

Panel B of Table 4 displays estimates for Equation (9). For the non-REIT sample, the combined estimate -3.173 ($\alpha_3 + \alpha_4 = -0.730 -2.443$) for depreciation expense suggests that depreciation *is* significantly related to stock returns, with a p-value of 0.023. By contrast, the corresponding estimate for the REIT group ($-0.151 - 0.752 = -0.903$) is not significantly different from zero ($p = 0.26$). Thus, there is some evidence that depreciation expense does have information content for capital-intensive firms, but not for REIT firms. Consistent with the change model (1), the estimate on $\Delta GAIN$ (-4.417 for non-REIT and 0.097 for REIT) is insignificant for either group.[15]

Turning to coefficient restrictions, the restriction that depreciation and gain (or loss) jointly have no information content ($\alpha_3 + \alpha_4 = \alpha_5 + \alpha_6 = 0$) can be rejected for the non-REIT group ($p = 0.057$), but not for the REIT sample ($p = 0.527$). Thus the levels-and-changes model for the REIT group yields results consistent with the results of the change model.

The returns model is a joint test of the accuracy of unexpected performance measures (ΔFFO, ΔDEP, $\Delta GAIN$) and their underlying associations with stock returns (Vincent 1999). Accordingly, measurement errors in proxies for performance potentially compromise the power of the tests (Kothari 1992; Kothari and Zimmerman 1995; Ryan and Zarowin 2003). A second interpretation is that depreciation expense lacks information content because it is highly predictable.[16]

Value Relevance of Accumulated Depreciation Based on Stock Price

Table 5, Panel A contains estimates for model (2) using weighted least squares (WLS, where variables are expressed on a per-share basis). Results (untabulated) are comparable when we estimate model (2) using OLS (where variables are scaled by lagged assets). As expected, both *NI* and the book value of equity are positively and significantly associated with stock price for both REITs and non-REITs.

Hypotheses 2a and 2b concern the value relevance of accumulated depreciation. For the non-REIT firms, the estimate for accumulated depreciation is no different from zero ($\beta_3 = 0.069$, $p = 0.639$). By contrast, the positive estimate for accumulated depreciation

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[15] For non-REITs, the large magnitude of coefficients on *GAIN* and $\Delta GAIN$ is in part due to some multicollinearity owing to high correlation between the two (Pearson correlation $= 0.860$; $p < 0.001$). This arises because a substantial proportion of the non-REITs' *GAIN* and $\Delta GAIN$ observations is zero—90.8 percent and 85.5 percent, respectively. The corresponding proportions for the REIT sample are 44.8 percent and 31.4 percent.

[16] Using changes as a proxy for unexpected depreciation assumes that depreciation follows a random walk, which can be unrealistic. Nevertheless, the Pearson correlation between current and lagged depreciation per share is 0.970 ($p < 0.001$) for the non-REIT control firms and 0.752 ($p < 0.001$) for the REIT sample. This suggests that the random walk model has predictive power and that investors can predict depreciation expense better for the non-REIT firms than for the REIT firms.

TABLE 5

Test of Value Relevance of Accumulated Depreciation Using Stock Price (Panel A) and Net Asset Value (Panel B) as the Dependent Variable

Panel A: Weighted Least Square (WLS) Estimates Using Per Share Variables

Model: $P_{i,t} = \beta_0 + \beta_1 NIps_{i,t} + \beta_2 BVEps_{i,t} + \beta_3 ACCUDEPps_{i,t} + \beta_4 DIVps_{i,t} + \beta_5 SGR_{i,t} + \beta_6 LEV_{i,t} + \sum_{i=2000}^{2004} \beta_i Y_i + \xi_{i,t}$

	Non-REITs			REITs			Tests of Coefficient Differences[a]	
	Coefficient	**t-stat.**	**p-value**	**Coefficient**	**t-stat.**	**p-value**	**t-stat.**	**p-value**
Intercept	8.461	2.401	0.017**	5.235	1.198	0.234	0.576	0.565
NIps	2.730	3.531	0.001***	7.649	6.841	0.000***	−3.630	0.000***
BVEps	1.065	7.610	0.000***	0.726	4.692	0.000***	1.630	0.105
ACCUDEPps	0.069	0.469	0.639	0.980	2.762	0.007***	−2.380	0.018**
DIVps	1.417	0.939	0.349	−1.005	−0.510	0.611	0.980	0.329
SGR	2.559	1.253	0.211	3.750	1.774	0.079*	−0.410	0.685
LEV	1.162	0.255	0.799	13.304	2.265	0.026**	−1.640	0.103
Y2000	−5.997	−2.948	0.004***	−19.784	−7.466	0.000***	4.130	< 0.001***
Y2001	−7.036	−3.600	0.000***	−19.403	−10.999	0.000***	4.700	< 0.001***
Y2002	−8.306	−4.109	0.000***	−18.112	−9.401	0.000***	3.520	0.001***
Y2003	−4.727	−1.492	0.137	−8.592	−5.636	0.000***	1.100	0.272
Y2004	−2.372	−1.425	0.156	−7.217	−5.878	0.000***	2.350	0.020**
n		468			468			
R^2		0.656			0.668			
Test of Coefficient Restrictions								
$\beta_2 = \beta_3$		4.01	< 0.001***		−0.62	0.535		

(*continued on next page*)

TABLE 5 (continued)

Panel B: Weighted Least Square (WLS) and Ordinary Least Square (OLS) Estimates Using Net Asset Value (NAV) as the Dependent Variable for the REIT Sample

Model: $NAVps_{i,t} = \beta_0 + \beta_1 NIps_{i,t} + \beta_2 BVEps_{i,t} + \beta_3 ACCUDEPps_{i,t} + \beta_4 DIVps_{i,t} + \beta_5 SGR_{i,t} + \beta_6 LEV_{i,t} + \xi_{i,t}$

	WLS Estimates (per share)			OLS Estimates (scaled by lagged assets)		
	Coefficient	**t-stat.**	**p-value**	**Coefficient**	**t-stat.**	**p-value**
Intercept	−45.285	−2.740	0.011**	−0.147	−0.374	0.712
NIps	4.727	1.952	0.061*	2.227	1.163	0.254
BVEps	1.125	2.890	0.007***	0.545	1.549	0.133
ACCUDEPps	1.062	1.719	0.097*	0.839	2.111	0.044**
DIVps	−0.589	−0.253	0.802	2.073	1.353	0.187
SGR	−2.310	−0.177	0.861	0.166	0.518	0.609
LEV	80.935	3.077	0.005***	0.779	1.751	0.091*
n		55			55	
R^2		0.526			0.155	
Test of coefficient restrictions						
$\beta_2 = \beta_3$		0.07	0.943		−0.57	0.575

(*continued on next page*)

TABLE 5 (continued)

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*, **, *** Denote two-tailed significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

[a] Tests of coefficient differences are based on a stacked regression where both REIT and non-REIT observations are pooled and each group has separate intercept and slope coefficients.

Reported estimates in Panel A are from pooled regressions over 2000–2005 for both the REIT and non-REIT firms. Estimates in Panel B are based on the REIT sample that has the NAV data.

Standard errors are based on Huber-White corrections clustered by firm.

Variable Definitions:

P = stock price per share at annual earnings announcement date adjusted for stock splits;
NIps = GAAP net income per share adjusted for stock splits;
BVEps = book value of equity per share adjusted for stock splits;
ACCUDEPps = accumulated depreciation and amortization per share adjusted for stock splits;
DIVps = dividend per share adjusted for stock splits;
SGR = sales growth;
LEV = leverage ratio;
NAVps = net asset value per share adjusted for stock split; and
Y2000–Y2004 = indicator variables that equal 1 for the corresponding fiscal year, and 0 otherwise.

See Table 1 for precise definitions of the variables.

for REIT firms ($\beta_3 = 0.980$) indicates that stock price for REITs is significantly associated with accumulated depreciation ($p = 0.007$). A pair-wise test of coefficient difference (column 3) confirms that the value relevance of accumulated depreciation for REITs is significantly greater than the value relevance of non-REITs ($p = 0.018$), notwithstanding that non-REITs are as capital-intensive as the REIT firms. Furthermore, a test of coefficient equality fails to reject the hypothesis that the coefficient for the book value of equity and the coefficient for accumulated depreciation are equal ($\beta_2 = \beta_3$) for the REIT group ($p = 0.535$), whereas such a restriction is confidently rejected for the non-REIT group ($p < 0.001$).

In sum, the evidence supports that investors find accumulated depreciation to be value-relevant (beyond GAAP net income and book value of equity) for REITs but not for non-REITs. Moreover, the evidence of coefficient equality between book value of equity and accumulated depreciation supports the REIT industry's position that GAAP depreciation distorts the underlying economic substance to a greater extent for REITs than for other industries. That is, FFO is a performance measure unique to the REIT industry.

Having found that accumulated depreciation correlates with equity prices, we provide additional support in a nonmarket setting by using the NAV estimates provided by REIT analysts.[17] According to NAREIT, NAV represents net "market value" of a company's assets less all liabilities and obligations.[18] REIT analysts attempt to estimate the market value of each REIT property based on its condition and past performance. Thus, the NAV of a REIT firm is the sum of the individual market values of properties less all liabilities.

Owing to tedious procedures involved in estimating NAV, REIT analysts typically provide these estimates for large firms only. We hand-collect NAV estimates provided by Green Street Advisors and published in *Forbes* (Fitch 2002; Fitch and Badenhausen 2003; Kump and Fitch 2005). Specifically, during the span 2001–2004, we obtain a total of 91 NAV estimates, of which 55 had complete data. We then estimate specification (2) using NAV per share (*NAVps*), instead of equity price per share, as the dependent variable. Panel B of Table 5 reports WLS (column 1) and OLS (column 2) estimates for the REIT firms. NAV estimates are unavailable for non-REITs.

The estimates affirm conclusions based on the price-level regression reported in Panel A of Table 5. Specifically, the WLS and OLS estimates (1.062 and 0.839, respectively) for *ACCUDEPps* are positive, and they are significant at the 10 percent and 5 percent level respectively. Furthermore, the equality between book value of equity and accumulated depreciation cannot be rejected ($p = 0.943$ and $p = 0.575$, respectively). In sum, the levels regression using NAV reinforces the findings from the regression using stock price.

Tests in the Non-Stock Market Setting

Accounting versus Economic Depreciation

We test H3a and H3b using observations with nonzero gain or loss from property sales. We identify 258 such observations in the REIT sample, whereas only 43 exist in the non-REIT control sample, suggesting that non-REIT assets are less frequently sold. Because such a substantial difference in sample size potentially affects statistical power and compromises comparisons, we expand the non-REIT sample to all firms in industries with mean fixed asset ratio exceeding 0.5. We obtain 300 non-REIT firm-years of nonzero gains or losses from this expanded sample.

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[17] We thank an anonymous referee for suggesting this test.

[18] The definition was obtained at the website of NAREIT at: http://www.reit.com/AllAboutREITs/GlossaryofREITTerms/tabid/62/Default.aspx.

Panel A of Table 6 reports estimates for model (7). Regression estimates indicate a substantial difference between REITs and non-REITs. In particular, the REIT sample yields a positive estimate for λ_1, suggesting that accounting depreciation exceeds economic depreciation for REITs. The point estimate 0.352 for λ_1 ($= 1 - \alpha_E/\alpha_A$) implies that the average accounting depreciation for the properties sold is about 54.3 percent higher than the average economic depreciation ($\alpha_E/\alpha_A = 0.648$, or $\alpha_A/\alpha_E = 1.543$) for the REIT industry. By contrast, the non-REIT sample yields a substantially smaller parameter estimate of 0.026 ($t = 6.915$), which suggests that the average accounting depreciation for the properties sold is only 2.7 percent higher than the average economic depreciation ($\alpha_E/\alpha_A = 0.974$, or $\alpha_A/\alpha_E = 1.027$). The pair-wise tests (column 3) indicate that λ_1 for REITs is significantly higher than for non-REITs, suggesting that the GAAP depreciation understates the value of REIT properties to a greater extent than the value of non-REITs.

These results are consistent with the REITs' claim that, compared to other industries, GAAP depreciation systematically exceeds economic depreciation and consistently understates book values of properties.[19] Because samples are restricted to observations where properties are sold, the test is open to a potential selection bias that may require Heckman-style corrections (Heckman 1979). To our knowledge, however, no existing theory predicts decisions to sell, rather than to hold, REIT or non-REIT assets.[20] As a result, we do not implement the Heckman approach.

The Ability of Accumulated Depreciation to Predict Future Revenues

Panel B of Table 6 reports regression estimates for model (8) for H4. Recall that the null hypothesis is that accumulated depreciation has no predictive content for future benefits because it represents, by definition, an unproductive portion of fixed assets.

Not surprisingly, current NOA are significantly related to future revenues for both groups. By contrast, accumulated depreciation is positively related to future revenues for the REIT group ($\gamma_2 = 0.576$, $t = 7.425$), but not for the non-REIT group ($\gamma_2 = 0.046$, $t = 0.233$). Thus, accumulated depreciation has a stronger positive correlation with future revenue for REITs than for non-REITs, again supporting the assertion that REIT accounting depreciation overstates economic depreciation.

Sensitivity Analysis

We execute additional analyses to ascertain whether results reported in this study are subject to alternative explanations and procedures.

Analysts' Forecast Errors as a Proxy for Unexpected FFO

We compute *FFO* indirectly from *NI*, ignoring miscellaneous reconciling items between *FFO* and *NI*. If measurement errors due to miscellaneous items are correlated with unexpected depreciation, then the coefficient for depreciation can be biased. To gain insight on the magnitude of measurement error, we re-estimate model (1) using forecasted and actual *FFO* for REITs from I/B/E/S.[21] We use analysts' forecasts issued after the earnings release for year $t-1$ but before the start of the fourth month of year t. Thus, unexpected *FFO* is actual *FFO* minus median analysts' *FFO* forecast (Vincent 1999). Untabulated results show that the mean (median) difference between the *FFO* per share used in this study and the actual *FFO* collected from I/B/E/S is 7.5 percent (6.7 percent). The magnitude of our

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[19] Including a lagged value of *GAIN* in model (7) produces comparable results.

[20] It is equally plausible, e.g., that REITs properties are sold because they are nonperforming or because their selling value exceeds expected future rental income.

[21] I/B/E/S does not publish actual or forecasted FFO for non-REIT firms.

TABLE 6
Tests of Value Relevance of Accumulated Depreciation Using Non-Stock Market Data for REITs and Non-REITs Firms

Panel A: Tests of Association between Accumulated Depreciation and Gain (or loss) from Sales

Model: $GAINps_{i,t} = \lambda_0 + \lambda_1 ACCUDEP\_SOLDps_{i,t} + \upsilon_{i,t}$

	Non-REIT Sample[a]			REIT Sample[b]			Tests of Coefficient Differences[c]	
	Coefficient	t-stat.	p-value	Coefficient	t-stat.	p-value	t-stat.	p-value
Intercept	0.022	0.841	0.401	0.159	1.598	0.114	−1.331	0.184
ACCUDEP_SOLDps	0.026	6.915	0.000***	0.352	2.700	0.008***	−2.51	0.013**
n		300			258			
R^2		0.480			0.230			

Panel B: The Ability of Accumulated Depreciation in Predicting Future Revenues for the Matched-Pair Sample

Model: $SALEps_{i,t} = \gamma_0 + \gamma_1 NOAps_{i,t-1} + \gamma_2 ACCUDEPps_{i,t-1} + \omega_{i,t}$

	Non-REIT Sample			REIT Sample			Tests of Coefficient Differences[c]	
	Coefficient	t-stat.	p-value	Coefficient	t-stat.	p-value	t-stat.	p-value
Intercept	2.096	0.748	0.455	0.893	1.470	0.145	0.419	0.675
$NOAps_{t-1}$	0.751	4.030	0.000***	0.135	4.342	0.000***	3.260	0.001***
$ACCUDEPps_{t-1}$	0.046	0.233	0.816	0.576	7.425	0.000***	−2.520	0.012**
n		468			468			
R^2		0.313			0.567			

(continued on next page)

TABLE 6 (continued)

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*, **, *** Denote two-tailed significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

[a] The non-REIT sample comprises all non-REIT firm-years reporting non-zero gains or losses, taken from industries whose mean fixed asset ratio exceeds 0.5 (300 observations).

[b] The REIT sample comprises all REIT firm-years reporting non-zero gains or losses (258 observations).

[c] Tests of coefficient differences are based on a stacked regression where both REIT and non-REIT observations are pooled and each group (REIT and non-REIT) has separate intercept and slope coefficients.

All reported estimates are based on Weighted Least Square (WLS) pooled regressions over 2000–2005.

Standard errors are based on Huber-White corrections clustered by firm.

Variable Definitions:

GAINps = gain (or loss) from sale per share;
ACCUDEP_SOLDps = accumulated depreciation of sold properties per share;
SALEps = net sales per share;
NOAps = net operating assets per share; and
ACCUDEPps = accumulated depreciation and amortization per share.

See Table 1 for precise definitions of the variables.

estimation errors is comparable to the mean (median) error of 8.2 percent (6.6 percent) reported by Baik et al. (2008, 282).[22] Nevertheless, we estimate specification (1) for the REIT sample using analysts' *FFO* forecast error as a proxy for unexpected *FFO*. Untabulated results are comparable to the results in Table 4: the estimate for ΔFFO is positive and significant ($\alpha_1 = 2.506$, $p = 0.014$), while neither ΔDEP ($\alpha_2 = -0.063$, $p = 0.949$) nor $\Delta GAIN$ ($\alpha_3 = 0.095$, $p = 0.769$) is significantly related to stock returns. Furthermore, we fail to reject the coefficient restriction $\alpha_2 = \alpha_3 = 0$ ($p = 0.957$) but reject $\alpha_1 = -\alpha_2 = \alpha_3$ at the 10 percent significance level ($p = 0.075$).

Land as a Correlated Omitted Variable

The value of land, which is not subject to accounting depreciation, can appreciate over time even if the value of a physical structure depreciates. If so, an alternative explanation for the significant and positive estimate for accumulated depreciation is that stock price increases with land price, which, in turn, is correlated with accumulated depreciation. To investigate this possibility, we hand-collect book values of land from 10-K and annual reports.[23] We then augment specification (2) with book value of land per share (*LANDps*) to examine whether land serves as a correlated omitted variable.[24] We denote the coefficient estimate for *LANDps* as β_{Land}.

Untabulated results are comparable to the results reported in Table 5, Panel A. More specifically, we obtain, for the REIT group ($n = 468$), $\beta_3 = 0.860$ ($p = 0.016$) and $\beta_{Land} = 0.767$ ($p = 0.006$), and for the matched non-REIT group ($n = 141$),[25] $\beta_3 = -0.636$ ($p = 0.009$) and $\beta_{Land} = -0.042$ ($p = 0.868$). Interestingly, these results suggest that investors recognize appreciation of land for REITs, but not for non-REITs. Such a finding is consistent not only with our perspective that land is viewed as an integral part of a larger set of operating assets for non-REITs, but also with the perspective of Barth and Clinch (1998) who find that property revaluation is not significantly associated with share prices for any industry, except for nonbank financial firms.[26] More importantly, REIT accumulated depreciation remains value-relevant even after controlling for land.[27]

Analysis by Industry

To gain additional insight on the role of depreciation, we estimate models (2), (7), and (8) for other industries based on the Fama and French (1997) classification. To ascertain adequate sample size, we consider all industries with at least 200 firm-year observations for estimating models (2) and (8). Because non-REIT firms have relatively few property sales, we consider all industries with at least 30 observations for estimating model (7). Among the 48 Fama-French industries, 25, 23, and 24 industries satisfy the criteria for estimating models (2), (7), and (8), respectively. Results (untabulated) indicate that for models (2) ((7), (8)), only two (three, nine) non-REIT industries yield a positive coefficient on *ACCUDEPps* (*ACCUDEP_SOLDps*, *ACCUDEPps*) significant at the 5 percent level or better. Most importantly, only the REIT industry consistently shows positive and significant coefficients in all three specifications.

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[22] Among the 468 REIT firm-year observations, 353 have actual and forecasted FFO per share in the I/B/E/S database. The mean (median) ***absolute*** percentage error in our sample is 16.9 percent (10.2 percent), compared to 13.4 percent (10.5 percent) in Baik et al. (2008, 282). The Pearson correlation between the FFO per share used in this study and the actual FFO taken from I/B/E/S is 0.904 ($p < 0.001$).

[23] Compustat provides the book value of land for non-REIT firms but not for REIT firms. Therefore we hand-collected book values of land from the Schedule III–Real Estate and Accumulated Depreciation filed for each REIT firm. We do not rely on their balance sheets because in most cases, the land value disclosed in balance sheets represents only land held for future development, rather than land underneath the associated properties.

[24] We use the book value of land because the market value of land—which is a better proxy—is unavailable. The regression thus assumes that land price appreciation is about the same for different firms in the same period. For REITs, *LANDps* = hand-collected land value/(data25 * data27); for non-REITs, *LANDps* = data260/(data25 * data 27).

[25] Of the 468 matched non-REIT observations, only 141 disclose the book value of land in their 10-K and annual reports.

[26] REIT (SIC code 6798) is considered as a nonbank financial industry by Compustat.

[27] For the REIT group, we fail to reject the coefficient restriction $\beta_2 = \beta_3$ ($p = 0.322$), indicating that from the perspective of investors, accumulated depreciation has the same value relevance as the book value of equity. In contrast, we reject the coefficient restriction $\beta_2 = \beta_3$ ($p = 0.001$) for the non-REIT group.

VII. CONCLUSIONS

Prior research reports ambiguous results regarding the superiority of Funds from Operations (FFO) over net income (NI). Because depreciation is advanced by the REIT industry as the primary motive for promoting the use of FFO, we examine the information content and value relevance of depreciation and accumulated depreciation using both the returns and levels approaches. Our study advances existing research along three avenues. First, we use both capital market and non-stock market information to test for the value relevance of depreciation. In the capital market setting, we focus on value relevance of accumulated depreciation based on the levels approach, rather than on the information content of depreciation expense based on the returns approach. In the non-stock market setting, we derive and test for a predicted association between accumulated depreciation and gains from sales based on a theoretical relationship between accounting depreciation and economic depreciation. We also test whether REITs' accumulated depreciation explains future revenues, contrary to the notion that assets that are expensed would not produce future benefits. Second, we provide a conceptual background for why accounting depreciation is expected to be less relevant for REITs than for other operating companies. Third, following the logic that characteristics of REIT depreciation differ from the characteristics of non-REITs, we adopt a cross-industry research design that considers both REIT firms and non-REIT firms of comparable capital intensity. These comparative analyses offer insight regarding the relevance of financial accounting depreciation not only for REITs, but also for all industries.

Our analyses yield at least three new inferences. First, accumulated depreciation is value-relevant for REITs, but not for non-REITs, beyond GAAP net income and book value of net assets. The implication is that investors correctly recognize the understatement of real estate property value due to the application of depreciation. Differences between REIT and non-REIT in value relevance of accumulated depreciation are both economically and statistically significant.

Second, accumulated depreciation of properties sold is positively associated with gains (losses) from sales for REIT firms, whereas such a relationship is much weaker and less economically substantial for non-REIT firms. The evidence suggests that for REIT firms, GAAP depreciation systematically exceeds economic depreciation and understates book value.

Third, accumulated depreciation has predictive power for future revenues for REIT firms but not for non-REIT firms. The result is consistent with the interpretation that for REITs, the book value of revenue-generating properties is understated and, thus, is partially captured by accumulated depreciation.

As with most empirical studies, this study is subject to the limitation that it can be affected by potential correlated omitted variables or misspecification. As a whole, however, our findings provide relatively coherent and consistent evidence that GAAP depreciation misrepresents economic depreciation for REIT firms. Moreover, we find that the degree of misrepresentation is greater for REITs than for other comparable industrial companies.

Accordingly, the results support the REIT industry's position that depreciation distorts REIT property value and firm performance measures.

Given the current real estate crisis, an interesting question is whether results hold when more recent data are considered. We anticipate that our primary conclusion—that depreciation for REIT is not value-relevant—holds for two reasons. First, large fluctuations in real estate values are consistent with the conjecture that REIT properties are more appropriately viewed as investment, rather than operating, assets. Accordingly, scheduled, systematic depreciation is less relevant for REIT assets than for other industrial assets. Second, over a long period including the recent period of decline in real estate values (1977–2008), commercial properties on average have appreciated by approximately 1.71 percent annually.[28] Therefore, the gap between accounting depreciation and economic depreciation remains substantial. Nevertheless, considering the periods when real estate values decline, rather than appreciate, are potentially interesting avenues for future research.

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[28] This is calculated based on the NPI Return Index Report released by the National Council of Real Estate Investment Fiduciaries (NCREIF) as of first quarter of 2009.

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Auditor Liability and Client Acceptance Decisions

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ABSTRACT: The accounting profession has raised concerns that excessive liability exposure renders audit firms unwilling to provide audit services to risky clients, limiting the prospective clients' ability to raise external capital. We address this concern in a model in which the auditor evaluates the riskiness of the client before accepting the client engagement. We consider a setting in which a shift to stricter legal liability regimes not only increases the expected damage payments from the auditor to investors in case of audit failure, but also increases litigation frictions such as attorneys' fees. The main finding is that the relationship between the strictness of the legal regime and the probability of client rejection is U-shaped. Our model suggests that in environments with moderate legal liability regimes, the client rejection rate is lower than in environments with relatively strong or relatively weak legal regimes.

Keywords: *auditor liability; client acceptance decisions; client risk.*

I. INTRODUCTION

The audit profession has long argued that excessively burdensome legal liability imposed on auditors hinders capital formation by increasing the likelihood that audit firms will reject potential clients, particularly high-risk firms, leaving such firms with limited access to capital markets. For example, the International Federation of Accountants (1995, 7) states: "The legal liability climate in some countries is causing an increasing number of large firms to avoid high-risk audit clients and even entire industries ... Without audited financial statements ... start-up businesses may not be able to generate shareholder confidence. As a result, economic growth can be stymied." In a similar spirit, the Public Oversight Board (1993, 9–10) notes "firms are reportedly refusing to undertake the audits of such (risky) companies ... This poses a grave problem because it severely hampers the access of such companies to the credit and equity markets. This could significantly hamper

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the ability of small companies to grow, create jobs, and develop imaginative products and services."

The intuition behind these arguments is clear: all else equal, a greater legal liability makes audit firms unwilling to accept risky clients, reducing the prospective clients' ability to fund new projects. However, in equilibrium, a change in the legal environment will also have an impact on the audit fee. Auditors' legal liability for an audit failure represents a form of implicit insurance to outside investors. The insurance provided by the auditor enables the entrepreneur to raise capital from investors at lower cost. The entrepreneur, in turn, can use these savings to compensate the auditor for the greater liability risk, reducing the likelihood of client rejection. Thus, the equilibrium implications of increased auditor liability on client rejection rates are not as obvious as implied by the audit profession's arguments.

Our objective is to shed some light on the implications of the legal liability environment for the auditor's decision to accept or reject risky clients, the level of audit quality (given acceptance), and the level of the audit fee, in a setting in which the auditor spends costly resources to evaluate the prospective client prior to making the acceptance decision.

In particular, we consider a setting in which an entrepreneur requires capital to undertake a new project and seeks that capital through outside investors. The entrepreneur can ask an auditor to provide information about the new investment opportunity.[1] Because the potential client is new to the auditor, the auditor knows little about the client initially and undertakes an evaluation prior to accepting the engagement. There are two potential types of clients: good-types and bad-types. The client's type, together with the characteristics of the project, determines whether the project (if financed) will succeed. When the auditor devotes greater effort to the evaluation process, she is more likely to discover the client's type. Given our focus on potentially risky clients, such as small start-up firms, we assume that the auditor will not want to accept the client if she fails to discover the client's type (which, of course, also implies rejection if negative information about the client-type is learned).[2] This assumption allows us to study situations in which the auditor sometimes rejects clients with promising new investment opportunities (i.e., good-type clients), consistent with the concerns raised by the audit profession. Clearly, this concern is alleviated if the auditor spends more time and effort evaluating the client because higher effort lowers the likelihood that good-type clients are rejected and unable to obtain financing.

Our focus is therefore on the moral hazard problem of inducing the auditor to devote effort to the evaluation task. The auditor's incentive to evaluate the client depends not only on the legal liability environment, but also on the audit fee offered by the entrepreneur: the higher the fee, the greater is the value of becoming informed about client-type (compared to staying uninformed and rejecting the client) and the higher is the auditor's effort devoted to the evaluation process. Consequently, the fee offered to the auditor plays an important incentive role in our setting.

If the auditor accepts the client, then she proceeds with an audit that provides information about the new investment opportunity. The investors' decision of whether to finance the project is based on the information provided by the auditor. Although the auditor effectively screens out bad-type clients, the auditor still faces litigation (audit) risk because the new project may fail after the auditor issues an unqualified opinion.[3]

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[1] Equivalently, the auditor can be viewed as certifying the client's assertion about project quality.

[2] Arthur Andersen et al. (1992, 22) note: "Accountants are also practicing risk reduction. The six largest firms are attempting to reduce the threat of litigation by avoiding what are considered high-risk audit clients and even entire industries."

In order to investigate the effects of the litigation environment on the probability that good-type clients get rejected, we consider three components of that environment: (1) the strictness of the legal liability regime, which is interpreted as the probability that the auditor will be sued and found liable after an audit failure, consistent with Shleifer and Wolfenzon (2002) and Choi et al. (2008); (2) damage payments from the auditor to investors in case of a successful lawsuit against the auditor; and (3) other litigation costs incurred by the auditor such as criminal penalties, attorney fees, or reputation loss. These latter costs are not recovered by investors and are, for clarity, labeled "litigation frictions."[4] In our setting, stricter legal liability regimes lead to both larger expected damage payments to investors and larger expected litigation frictions.

We show that under reasonable assumptions about the level of expected damage payments, an increase in any of these litigation components results in an increase in both audit quality and the equilibrium audit fee. This relationship is consistent with empirical evidence by Choi et al. (2008), Venkataraman et al. (2008), and Seetharaman et al. (2002). However, when considering the probability of client rejection, it is important to carefully distinguish between the three components of the liability environment.

We first show that an increase in the potential damage payments to investors leads to a reduction (not an increase) in the client rejection rate. A higher expected damage payment implies that the entrepreneur has to offer the auditor a larger audit fee. Otherwise, the audit engagement would become less attractive to the auditor, which would lead to a lower evaluation effort and hence a higher rejection rate. However, the increase in the audit fee does not involve a real cost to the entrepreneur. If investors expect a larger damage award from the auditor in case of an audit failure, then investors are willing to give the entrepreneur better financing conditions. The entrepreneur, in turn, can use these savings to compensate the auditor for the increased liability exposure. We call this the *triangle effect*. Hence, a change in the damage payment has no *direct* effects on the evaluation effort and the rejection rate. However, there is also an indirect effect, since a larger potential damage award induces the auditor to adopt an audit of higher quality (after accepting the client), which delivers more accurate information about the investment project and hence leads to improved investment decisions. The anticipation of a better investment decision increases the value of the entrepreneur's investment opportunity in the initial stage. Since this investment opportunity is lost if the auditor rejects the engagement, the entrepreneur is more eager to attract the auditor. To do this, the entrepreneur increases the audit fee by an amount that is larger than the increase in the auditor's expected damage payment, which results in a higher evaluation effort and a lower rejection rate.

If, on the other hand, litigation frictions increase, then the above result is reversed; that is, the client rejection rate increases. When litigation frictions are higher, the auditor will find the engagement with the client less attractive and hence will have a weaker incentive to carefully evaluate the client, which increases the rejection rate. Of course, the client can counteract this negative effect by offering a larger audit fee, but in this case a real cost is involved because the triangle effect does not hold. As a result, the equilibrium rejection rate increases with higher litigation frictions.

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[3] Smith et al. (2000) also model a two-stage audit process, where the auditor first evaluates internal control strength and then performs substantive tests for fraud. Similar to our model, the first-stage evaluation in Smith et al. (2000) only provides information about the manager's type and not whether fraud was actually committed. However, Smith et al. (2000) do not consider an endogenous audit fee or the auditor's decision to accept or reject the client.

[4] Radhakrishnan (1999) also separates audit litigation penalties into payments to investors and payments to lawyers (he calls the latter "recovery frictions"). However, he does not consider the effects of these penalties on the auditor's client acceptance decision.

Because a shift in the strength of the legal regime affects both the expected damage payments to investors as well as expected litigation frictions, a change in the legal regime involves two opposing effects. Depending on which effect is stronger, a change in the legal regime either increases or decreases the probability of client rejection. In particular, we show that the relationship between the strength of the legal liability regime and the client rejection rate is U-shaped. Our model therefore predicts that clients are less likely to be rejected in environments with moderate legal regimes, as compared to environments with relatively strong or relatively weak legal regimes.

The literature concerned with auditors' decisions to accept or reject potential clients is mainly survey and empirical research.[5] Most formal models of auditor-client interactions focus on the effects of changes in the legal liability environment on the auditor's incentive to provide high quality audits.[6] Similar to our study, Deng et al. (2008) consider the effects of auditors' legal liability on the efficiency of investment decisions. In Deng et al. (2008), the auditor trades off the cost of Type I and Type II errors when deciding whether to provide a qualified or unqualified opinion. An increase in the auditor's legal liability causes the auditor to interpret audit information more conservatively, thus incorrectly rejecting client reports more often. The upshot is fewer audit failures but an increased likelihood that favorable investment projects will not be funded. In contrast, in our setting, the source of potential underinvestment is the auditor's moral hazard problem with respect to the client-evaluation task. Both the legal regime and the audit fee chosen by the entrepreneur affect the auditor's incentive to assess the client's type prior to the acceptance/rejection decision. Because the audit fee will be optimally adjusted to changes in the legal regime, a stricter legal regime does not necessarily lead to more client rejections (i.e., fewer investments).

In Section II, we develop the model. Section III analyzes a benchmark situation where the auditor's choices of evaluation effort and audit quality are observable and contractible. In Section IV, we consider our main setting with unobservable effort choices and derive the optimal audit effort, client evaluation effort, and audit fee. In Section V, the effects of changes in the auditor's legal environment are described. Section VI considers the effects of variations in the riskiness of the project. Section VII concludes.

II. MODEL

Consider a setting with three risk-neutral parties: an entrepreneur, outside investors, and an auditor. The entrepreneur needs capital $I > 0$ to undertake a new project. In order to obtain the required capital, the entrepreneur sells $\beta \in [0,1]$ proportion of the project's pay-off to outside investors (β is derived endogenously). If the project is financed, then it generates cash flows of $x = X > I$ if it succeeds and $x = 0$ if it fails. If the entrepreneur is unable to obtain the required capital I from investors, then the project is not undertaken and the entrepreneur receives zero pay-off.

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[5] See, e.g., Asare et al. (1994), Huss and Jacobs (1991), Johnstone and Bedard (2003, 2004), and Johnstone (2000).

[6] See, e.g., Dye (1993, 1995), Chan and Pae (1998), Chan and Wong (2002), Hillegeist (1999), Narayanan (1994), and Schwartz (1997).

To focus on two types of activities engaged in by auditors, client evaluation versus auditing, we assume that the probability of project success is dependent on both the type of the client and the underlying characteristics of the project. There are two types of entrepreneur, a bad-type, $T = B$, and a good-type, $T = G$. The *a priori* probability of a good-type is denoted by $p \in (0,1)$. The type of the project is also either bad or good and is denoted by $t \in \{b,g\}$. The *a priori* probability of a good-type project is $\theta \in (0,1)$. We refer to $(1 - \theta)$ as *project risk.* The types of the client and the project determine the probability of project success. For simplicity, we assume that the project succeeds if and only if $T = G$ and $t = g$. Both T and t are not known *ex ante* to any players, including the entrepreneur.[7] We assume that without further information, investors are not willing to finance the project, i.e., $p\theta X - I < 0$.

The entrepreneur offers the auditor a noncontingent audit fee, denoted W, for audit services. The auditor is free to accept or reject the audit engagement. Before making this decision, the auditor devotes effort $e \in [0,1]$ to evaluate the client's type T. We assume that effort e represents the probability that the auditor observes a perfect signal about the client's type. With probability $(1 - e)$, the auditor obtains no additional information. That is, after evaluating the client, the auditor either knows the client's type with certainty or has no better information than before the evaluation. The auditor's private cost of effort e is $c(e)$, with $c(0) = 0$, $c'(e) > 0$, $c''(e) > 0$, and $c'(0) = 0$.

In order to emphasize the importance of the client-evaluation task we assume that the auditor prefers to reject the client if she fails to discover the client's type (which also means that the auditor rejects if she learns the client's type is bad). Intuitively, this assumption is satisfied if the likelihood of a bad-type client is relatively high and the legal liability environment and the auditor's reputational concerns are sufficiently strong.[8] Due to this assumption, and consistent with concerns raised by the audit profession, the auditor will sometimes reject clients with promising new investment opportunities (i.e., good-type clients). This problem is alleviated if the auditor spends more effort on evaluating the client because greater effort reduces the probability that the auditor remains uninformed about client-type, and hence reduces the chance that good-type clients get rejected. Our focus is therefore on the moral hazard problem of inducing the auditor to carefully evaluate the client.

As a consequence of our assumption that the auditor accepts only good-type clients, we naturally consider clients that are viewed as problematic for auditors, i.e., firms that are high risk, about whom little is known, and for whom the failure potential is great. For these firms, the auditor's evaluation effort and conclusion are particularly important because the auditor is more likely to reject the engagement if evaluation results are not unambiguously positive.

If the auditor accepts the new client, then the auditor proceeds to conduct an audit. In order to distinguish the audit activity from the evaluation activity, we assume that the audit only provides information about the project-type t (but not about the client-type T).[9] The auditor chooses audit effort a, which determines the quality of the audit. We assume that effort a represents the probability that the auditor reports the project-type as $\hat{b}$, given that the true project-type is b. When the project-type is g, the auditor reports $\hat{g}$ with certainty.[10] Hence, for an imperfect level of auditing ($a < 1$), the auditor will sometimes report the project-type to be $\hat{g}$ even though the true type is b.[11] Audit cost is denoted by $k(a)$, with $k(0) = 0$, $k'(a) > 0$, and $k''(a) > 0$.

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[7] The assumption that the entrepreneur has no private information is common in the auditing literature; see, for example, Dye (1993, 1995), Schwartz (1997), Chan and Pae (1998), and Chan and Wong (2002).

[8] The conditions for which this assumption is satisfied and related proofs are available upon request.

[9] Alternatively, the auditor may be viewed as obtaining information about the veracity of the client's assertions about the project-type. In such a setting, the client would always claim that the project-type is g and the auditor would provide either a qualified or unqualified opinion.

We assume that investors behave competitively, in the sense that they make zero profits. Given the assumption that $p\theta X - I < 0$, investors are unwilling to finance the project in the absence of an audit. Moreover, if the auditor accepts the audit engagement and reports $\hat{b}$, then investors will again not provide capital I, as it is clear that the project will fail. In these cases, the project is abandoned, and the game ends. If the auditor accepts the engagement and issues a report $\hat{g}$, then investors are willing to finance the project in exchange for a fraction $\beta \leq 1$ of the project's final cash flows.

If the project is implemented and succeeds, the game ends. An audit failure occurs if the auditor issues a favorable report and the project fails. In this case, the entrepreneur is bankrupt and unable to pay any damages to investors.[12] The investors' only recourse to recover their investment is to sue the auditor for an incorrect report (i.e., an audit failure). The auditor's expected litigation cost in case of an audit failure is given by $L = s(D + F)$, where D is the damage payment to investors and $F \leq D$ is the litigation friction, such as the cost of attorney fees.[13] The parameter $s \in [0,1]$ reflects the probability that the auditor is sued and found liable in case of an audit failure and is referred to as the strictness of the legal liability regime.[14]

We assume that the expected damage payment to investors in case of an audit failure is less than the investors' loss. Since the investors' loss is the initial investment I, this assumption implies that $sD < I$. Our focus on $sD < I$ seems reasonable for two reasons. First, if expected damages exceed investment losses, $sD > I$, investors are willing to invest in a project at terms that yield negative net pay-offs if it succeeds ($\beta X - I < 0$) and positive net pay-offs if it fails ($sD - I > 0$).[15] Thus, *ex post*, investors are better off if projects fail, which would generate perverse incentives to sabotage successful projects. Second, the empirical evidence regarding litigation outcomes in the U.S. following audit failures indicates that damage payments received from auditors are substantially smaller than investor losses.[16] This evidence is consistent with casual observation that, generally, investors do not applaud audit failures. In most litigation environments, the auditor's maximum legal exposure to investors would be no greater than the loss the investors incur, i.e., $D \leq I$ (also see the discussion in Schwartz [1997]).

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[10] A similar audit technology is assumed in Dye (1993, 1995), Schwartz (1997), Chan and Pae (1998), Hillegeist (1999), Radhakrishnan (1999), Chan and Wong (2002), and others.

[11] Consistent with prior audit models (see footnote 10), we assume that the auditor reports in a manner consistent with audit evidence; that is, we ignore any possible moral hazard issue related to the audit report. This assumption can be justified by an appeal to a setting in which the auditor must support, through verifiable evidence, his report. In contrast, if the auditor could choose any report with impunity, then in the single period model we study, the auditor would always accept the client (and the fee), choose to exert no audit effort, and report $\hat{b}$, thus avoiding shareholder litigation. Of course, the market for audits could not be sustained in such an environment.

[12] The assumption that the entrepreneur provides no insurance to investors is consistent with assumptions in Dye (1993, 1995), Chan and Pae (1998), Schwartz (1997), Deng et al. (2008), and others.

[13] The assumption that $F \leq D$ is relevant only for Proposition 6 in Section V where we investigate the effects of changes in s. We place this upper bound on F because for large levels of F the legal environment becomes dysfunctional in the sense that both the entrepreneur and the auditor are best off if the litigation risk is zero, i.e., $s = 0$.

[14] Alternatively, one could assume that the auditor is liable for damages only if the court concludes that he is "negligent." This negligence scenario can be modeled by assuming that the expected litigation cost in case of an audit failure is $s(h(a)D + F)$, where s reflects the probability that investors sue the auditor and $h(a)$ is the probability that the auditor is found negligent, with $h'(a) < 0$. Our qualitative results are robust to such a modeling change (proof is available upon request).

[15] This can be seen by setting the investors' expected utility (stated in (3)) equal to their reservation utility of zero.

As will become clear later, litigation frictions ultimately reduce the value of the entrepreneur's investment opportunity. To ensure that the expected value of the investment opportunity is positive for a good-type client even if the audit quality is low ($a = 0$), we assume that $\theta X - I - (1 - \theta)sF > 0$.

Since the auditor accepts the client only after learning that the client is a good-type, the *ex ante* probability of client acceptance is given by ep.[17] The expected pay-offs for the auditor, U^A, the entrepreneur, U^E, and the investor, U^I, can be stated as:

$$U^A(e,a) = ep[W - (1 - a)(1 - \theta)L - k(a)] - c(e), \tag{1}$$

$$U^E(e,a) = ep[\theta(1 - \beta)X - W], \tag{2}$$

$$U^I(e,a) = ep[\theta(\beta X - I) + (1 - \theta)(1 - a)(sD - I)]. \tag{3}$$

The reservation utility of the three players is normalized to zero. Investors are willing to finance the project if and only if the auditor accepts the audit engagement and issues a favorable report about project-type. If the project-type is g (which occurs with probability θ), then the project is implemented and succeeds. In this case, the investors' pay-off is $\beta X - I$ and the entrepreneur receives $(1 - \beta)X$. If the project-type is b and the auditor fails to report $\hat{b}$ (which occurs with probability $(1 - \theta)(1 - a)$), then the project is implemented and fails. In this case, the entrepreneur is bankrupt, and the investors expect to obtain the damage award sD from the auditor.

The timeline of the model is as follows:

Stage 1: The entrepreneur requests an audit and offers the fee W to the auditor.

Stage 2: The auditor devotes effort e to evaluate the client and makes an acceptance/rejection decision based on the acquired information.

Stage 3: If the auditor declines the prospective client, the game ends. If she accepts the client, she conducts an audit and issues a report. In case of a favorable report, $\hat{g}$, investors finance the new project. In case of an unfavorable report, $\hat{b}$, the investors do not finance, and the game ends.

Stage 4: If the project is undertaken in stage 3, final cash flows x are realized. If the project succeeds, profits X are shared between the investors and the client based on the sharing rule β. If the project fails, investors sue the auditor in an attempt to recover damages.

III. BENCHMARK: THE FIRST-BEST SOLUTION

As a benchmark, it is helpful to consider the first-best solution, in which the auditor's effort levels e and a are observable and contractible. If effort levels are contractible, then the entrepreneur can implement any level through a forcing contract, in which the auditor is compensated for her effort cost only if she exerts the contracted level of effort. The entrepreneur's goal is to maximize his utility, subject to the constraints that the auditor and the investors receive their reservation utilities, i.e., $U^A = 0$ and $U^I = 0$. Substituting the participation constraints for the auditor and the investors into (2) yields the entrepreneur's maximization problem:

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[16] Palmrose (2005) reports that for a sample of 57 class actions filed against auditors during the period 1996–2001 (and resolved by mid-2002), actual auditor payments were substantially less than potential investor losses, represented by the decrease in equity over the class period.

[17] More precisely, the auditor will only accept a good-type client if the audit fee is sufficiently high. However, as shown later, this is always the case in equilibrium.

$$\max_{e,a} ep[\theta(X - I) - (1 - \theta)(1 - a)(I + sF) - k(a)] - c(e).$$

The optimal levels of audit effort and evaluation effort, denoted a^f and e^f, satisfy:

$$(1 - \theta)(I + sF) - k'(a) = 0 \tag{4}$$

and:

$$p[\theta(X - I) - (1 - \theta)(1 - a)(I + sF) - k(a)] - c'(e) = 0. \tag{5}$$

In order to ensure the auditor's participation, the entrepreneur needs to compensate the auditor for the expected litigation friction in case of an audit failure, sF. In contrast, the expected damage payment sD does not show up in the above problem, because sD is not a real cost to any player. Of course, the entrepreneur needs to compensate the auditor for the expected damage payment, sD. However, since investors are the beneficiaries of potential damages, the entrepreneur is able to recoup this outlay in the form of better financing conditions (i.e., a lower β). This situation is equivalent to a three-person game in which the players stand in a circle, each handing $10 bills to the player to the left. Clearly, changing the amount of money transferred does not make anyone better or worse off. We call this the *triangle effect*.

A higher quality audit helps to improve the investment decision in stage 3, in the sense that the project is implemented less often if the project-type is b. An improved investment decision is not only beneficial because it reduces the probability of wasting capital I for a bad-type project, but also because it reduces the probability of audit failure and hence the expected litigation friction. The larger the capital outlay, I, and the larger the expected litigation friction in case of audit failure, sF, the larger is the optimal audit effort a^f.

To understand Condition (5), note that the entrepreneur possesses a real option to invest in the new project in stage 1. The value of this real option for the good-type entrepreneur is captured by the term in square brackets in Condition (5). If the auditor rejects the client, then the entrepreneur loses the option to invest, since there will be no financing. The higher the value of the real option, the more important is the evaluation effort undertaken by the auditor because higher effort reduces the probability of rejection. Note that the stage 1 value of the real option to invest in the project depends on the anticipated audit quality in stage 3. For $a < a^f$, an increase in audit quality improves the value of the investment opportunity and hence the optimal level of evaluation effort, e^f.

IV. EQUILIBRIUM

Audit Quality

We begin the analysis by determining the auditor's optimal choice of audit quality, given that she has accepted a good-type client. To find the optimal effort a, the auditor solves:

$$\max_{a} W - (1 - a)(1 - \theta)L - k(a).$$

The optimal choice of a, denoted a^*, satisfies:

$$(1 - \theta)L - k'(a) = 0. \tag{6}$$

Clearly, the audit fee W has no impact on the quality of the audit, because W does not depend on the outcome of the audit. However, if the expected litigation cost L increases, then the auditor will have a stronger incentive to carefully audit the client. Note that for $I = sD$, the auditor implements the first-best audit quality, $a^* = a^f$. Due to our assumption that $sD < I$, the auditor under-invests in audit quality from a first-best perspective; that is, $a^* < a^f$.

Proposition 1: If the expected litigation cost, L, increases, then the auditor chooses a higher audit quality, a.

Thus, if any component of the auditor's litigation environment increases, then we expect an increase in audit quality for the clients accepted by the auditor. This prediction is generally consistent with evidence in Venkataraman et al. (2008).

Outside Investors

Outside investors are willing to finance the project only if the auditor accepts the engagement and issues a favorable report about the project. In this case, investors provide the required capital I in return for a fraction β of the project's cash flows. In order to determine β, we set the investors' *ex ante* utility (3) equal to their reservation utility (zero), which after rearranging yields:

$$\beta = \frac{\theta I + (1 - \theta)(1 - a^*)(I - sD)}{\theta X}. \tag{7}$$

The entrepreneur becomes better off if he has to give up a smaller fraction β in order to obtain the required capital. Keeping the audit effort fixed, the level of β declines when the damage award to investors D increases. Intuitively, since investors can expect a larger payment from the auditor in case of project failure, investors are willing to finance the project in exchange for a smaller fraction β of the project's cash flows.

A change in the damage award, D, also has an impact on the audit quality, a. There are two relevant effects associated with an increase in audit quality. First, a higher audit quality improves the investment decision in the sense that the project is implemented less often when the project-type is b. Second, a more diligent audit reduces the likelihood that investors obtain damage awards from the auditor after investing in the project. While the first effect reduces β, the second effect increases β. However, for $sD < I$, the former effect dominates the latter, implying that investors demand a lower fraction β of final cash flows if audit effort increases.

Proposition 2: The entrepreneur has to give up a smaller fraction β of final cash flows if the damage payment to investors, D, increases.

Client Evaluation

In stage 2, the auditor decides how much effort to devote to the client evaluation process. Assume that the audit fee offered by the entrepreneur is high enough to induce the auditor to accept a good-type client; that is, $W - (1 - a^*)(1 - \theta)L - k(a^*) > 0$. If this condition is not satisfied, then the auditor would remain uninformed and always reject the engagement. We show in the next subsection that this condition is satisfied in equilibrium.

The auditor's maximization problem is given by:

$$\max_{e} ep(W - (1 - a^*)(1 - \theta)L - k(a^*)) - c(e),$$

where a^* satisfies (6). The first-order condition for an optimal choice of e is:

$$p(W - (1 - a^*)(1 - \theta)L - k(a^*)) - c'(e) = 0. \tag{8}$$

Holding the audit fee constant, consider how a change in the auditor's expected litigation cost, L, affects the optimal choice of effort e. There are two effects, an indirect effect and a direct effect. The indirect effect occurs because a higher legal liability induces the auditor to choose a higher audit quality. Because the level of a is chosen optimally in equilibrium, by the envelope theorem, this indirect effect is only second order and, hence, is negligible. The direct effect of an increase in L on the auditor's choice of e is negative. To see this, note that accepting the client is associated with litigation risk for the auditor even if the client is a good-type. Hence, when the litigation exposure, L, increases, the strategy to acquire information and accept good-type clients becomes less attractive compared to the strategy to stay uninformed and reject the client. As a result, the auditor will exert less evaluation effort if the legal liability environment becomes tougher.

Keeping the liability environment fixed, a higher audit fee, W, induces the auditor to devote more effort to the evaluation process. When the audit fee increases, the strategy to remain uninformed and reject the client becomes less attractive, which increases the value of obtaining information. The audit fee, W, therefore plays an important incentive role in our setting.

Proposition 3: The auditor chooses a higher evaluation effort, e, if the expected litigation cost, L, declines (holding audit fee constant) and/or the audit fee, W, increases.

Since the auditor rejects the client if uninformed about the client's type, it follows that the probability of rejection is larger if the auditor chooses a lower evaluation effort. Let $R_G = p(1 - e)$ denote the probability that a good-type client is rejected and $R = (1 - p) + p(1 - e)$ the probability that the client is rejected independent of type. The next corollary directly follows from Proposition 3.

Corollary 1: The probability that the good-type client is rejected, R_G, and the overall rejection rate, R, increase if the expected litigation cost, L, increases (holding audit fee constant) and/or the audit fee, W, decreases.

The finding that clients are more likely to be rejected if the litigation environment is tougher is consistent with arguments advanced by the audit profession. However, as we show in the next subsection, this argument is incomplete because a shift in the legal liability regime has an effect on the equilibrium audit fee offered by the entrepreneur.

Audit Fee

In stage 1, the entrepreneur chooses the audit fee, W, offered to the auditor. The entrepreneur solves the following optimization problem:

$$\max_{W} pe(\theta X(1-\beta)-W), \tag{9}$$

subject to (6), (7), and (8). As shown below, the participation constraint for the auditor, $U^A \geq 0$, is always satisfied in equilibrium and hence can be ignored.

Lemma 1: The optimal level of W satisfies:

$$-e+[\theta(X-I)-W+(1-a)(1-\theta)(sD-I)]\frac{p}{c''(e)}=0, \tag{10}$$

where e and a satisfy (8) and (6), respectively.
Proof: See the Appendix.

As shown in the previous subsection, an increase in the audit fee, W, enhances the auditor's incentive to diligently evaluate the client before making the acceptance/rejection decision. A higher evaluation effort increases the likelihood that the auditor learns the client's type, reducing the probability that good-type clients are rejected. The (good-type) entrepreneur is therefore better off if the auditor devotes more effort to the evaluation process.

When choosing the optimal level of W, the entrepreneur takes into consideration that a larger audit fee increases the auditor's incentive to exert evaluation effort. The positive effect of a higher e on the entrepreneur's utility is captured by the term in square brackets in (10). This effect is traded off with the marginal cost of an increase in W.

Substituting (10) into (8) yields the equilibrium evaluation effort, denoted e^*, which satisfies:

$$p[\theta(X-I)-(1-a^*)(1-\theta)(I+sF)-k(a^*)]-ec''(e)-c'(e)=0. \tag{11}$$

The term in square brackets in (11) represents the value of the investment opportunity for a good-type client. Since the value of this investment opportunity is positive, the equilibrium evaluation effort is also positive, i.e., $e^* > 0$.[18]

Substituting (11) into (10) and rearranging yields:

$$W^*=(1-a^*)(1-\theta)L+k(a^*)+\frac{c'(e^*)}{p}. \tag{12}$$

The expected cost of the audit engagement, denoted AC, is composed of the expected litigation cost and the direct cost of auditing and is determined by $AC=(1-a^*)(1-\theta)L$

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[18] Recall that we assumed in Section II that $\theta X - I - (1-\theta)sF > 0$, which implies that the term in square brackets in (11) is positive for all $a \leq a^f$.

$+ k(a^*)$. Equation (12) shows that the equilibrium audit fee, W^*, is strictly higher than the expected cost of the audit engagement, AC, which justifies our previous assumption that $W > AC$. Thus, if the auditor learns that the client-type is good, she will accept the engagement. The result that the auditor obtains a wage that exceeds the expected audit cost deserves some attention because it differs from the standard assumption in the literature that the auditor, when accepting the engagement, earns zero expected profits in equilibrium.

The fee offered to the auditor is higher than AC because in our setting the entrepreneur has to overcome an additional moral hazard problem: the auditor must be induced to expend evaluation effort prior to the acceptance/rejection decision. If the fee just compensates the auditor for the expected cost of the engagement, i.e., if $W = AC$, then the auditor has no incentive to expend costly evaluation effort. By choosing $W > AC$, the entrepreneur makes it attractive for the auditor to acquire information about the client instead of staying uninformed and rejecting the engagement.

Note that from the result that $W^* > AC$, it follows that $U^A(e^*,a^*) > 0$, that is, the auditor is able to obtain an economic rent in equilibrium. To see this, recall that the auditor chooses the effort level that solves $\max\limits_{e} U(e,a^*) = ep(W^* - AC) - c(e)$. For $W^* > AC$, and given $c'(0) = 0$, the marginal benefit of effort at $e = 0$ is larger than the marginal cost, implying that $e^* > 0$ and $U(e^*,a^*) > 0$. To determine the auditor's rent, substitute the equilibrium wage, W^*, as specified in (12) into the auditor's equilibrium utility, $U^A(e^*,a^*)$, as specified in (1), which yields $U^A(e^*,a^*) = e^*c'(e^*) - c(e^*)$. Since $c(e)$ is concave in e, it follows that $U^A(e^*,a^*) > 0$ for $e^* > 0$. The larger the induced equilibrium effort, e^*, the larger is the auditor's rent.

The auditor's ability to earn rents results from the moral hazard problem with respect to evaluation effort, combined with the constraint that all payments to the auditor must be non-negative; that is, the auditor cannot be punished if she rejects the client. For the sake of argument, suppose that negative payments to the auditor are feasible. Then, the evaluation effort incentive problem could be alleviated by punishing the auditor whenever she rejects the client. In this situation, the entrepreneur could induce evaluation effort without granting the auditor any rents in expectation. Of course, we do not believe that such a scenario is reasonable because penalties for rejecting clients cannot be enforced. Thus, our setting has similarities to a standard moral hazard setting where a principal contracts with an agent who is protected by limited liability. To induce effort, the principal offers a bonus for high outcomes but cannot punish the agent for low outcomes, implying that the agent enjoys a rent in expectation (e.g., Innes 1990; Laffont and Martimort 2002).

V. THE ROLE OF THE LEGAL ENVIRONMENT

This section analyzes how a change in the components of the legal liability environment affects the equilibrium outcome of the game. In particular, we analyze how an increase in the strictness of the legal regime, s, damage payments, D, and litigation frictions, F, affect the equilibrium levels of audit fee, W, evaluation effort, e, rejection rate, R, and the utilities of the entrepreneur, U^E, and the auditor, U^A.

In order to simplify the exposition, it is helpful to consider specific cost functions for the auditor. We therefore assume for the remainder of the analysis that $k(a) = 0.5ka^2$ and $c(e) = 0.5ce^2$, where k and c are sufficiently large to ensure interior solutions.

Change in Damage Payment *D*

We start by analyzing the equilibrium effects of a change in the damage payment D to investors. A larger potential damage payment induces the auditor to choose a higher audit quality, a. However, as a first step, it is helpful to consider a benchmark setting, in which audit quality remains fixed (i.e., does not change with D). Assuming a is fixed, an increase in damages D has two immediate effects: first, it reduces the fraction β the entrepreneur needs to sell to investors in order to obtain capital I and, second, it renders the engagement with the client less attractive to the auditor, implying a lower level of evaluation effort and a higher rejection rate (as discussed in the previous section). The optimal response of the entrepreneur is to counteract the decline in evaluation effort by increasing the audit fee by $(1 - a)(1 - \theta)s\Delta D$, where ΔD is the change in the damage payment (see (10)). By doing so, the entrepreneur restores the auditor's incentive to evaluate the client to its original level. In other words, in equilibrium, a change in the damage award has no direct effect on the auditor's evaluation effort and the rejection rate. This result can also be verified by observing that the equilibrium effort level e^*, determined by (11), does not directly depend on D (recall that we are assuming here that a^* is not affected by a change in D). Of course, this result is similar to the triangle effect outlined in the benchmark case.

However, a change in D does have real effects if one takes into consideration that an increase in D induces the auditor to conduct a more careful audit. Note that the total surplus generated in case of a good-type client is a function of audit quality and is determined by $\theta(X - I) - (1 - \theta)(1 - a)(I + sF) - 0.5ka^2$. Three effects are associated with an increase in audit quality. First, a more careful audit is associated with a higher audit cost. Second, an increase in audit quality reduces the probability of an audit failure and hence reduces the expected litigation friction. Finally, the provision of more accurate information results in an improved investment decision in the sense that the project is implemented less often when the project-type is b. Given the assumption that $sD < I$, the benefits associated with an increase in audit effort outweigh the additional costs, such that the audit effort moves closer to the first-best level a^f. An increase in audit quality therefore increases the total surplus generated from a good-type client. Since this surplus is lost if the auditor rejects the engagement, the entrepreneur is more eager to attract the auditor. To achieve this, the entrepreneur chooses an audit fee that over-compensates the auditor for the increase in the expected damage payment. This increase in the fee provides the auditor with stronger incentives to evaluate the client and results in a lower rejection rate.

Proposition 4: An increase in the damage payment, D, has the following equilibrium effects:

(i) the audit fee, W, increases,
(ii) the evaluation effort, e, increases,
(iii) the probability that the good-type client gets rejected, R_G, and the overall rejection rate, R, decrease, and
(iv) the entrepreneur's and the auditor's expected pay-offs, U^E and U^A, increase.

Proof: See the Appendix.

The result that an increase in the potential damage payment to investors is associated with a decline (and not an increase) in the client rejection rate is counterintuitive. The important point here is that the auditors' willingness to provide audit services not only depends on the litigation environment, but also depends on the audit fees that result from those engagements; and the legal environment and the equilibrium audit fees are not independent.[19]

Change in Litigation Friction F

We now consider the effects of a change in the litigation friction, F. Clearly, a larger F induces the auditor to adopt an audit of higher quality. However, there is also a negative effect associated with a larger litigation friction that outweighs this positive effect. The expectation of a higher litigation friction renders the engagement with the client less attractive to the auditor and hence lowers her incentive to expend effort to evaluate the client. This reduction in evaluation effort, in turn, results in a higher likelihood of client rejection. The entrepreneur can counteract this effect by increasing the audit fee, but this is costly. The important difference between a change in F and a change in D is that the triangle effect does not hold for the former. That is, the cost F is not recovered by investors and hence not passed on to the entrepreneur. Put simply, a larger level of F increases the cost of hiring the auditor and hence the cost of implementing the project. This leads to the next proposition.

Proposition 5: An increase in the litigation friction, F, has the following equilibrium effects:

(i) the audit fee, W, increases,
(ii) the evaluation effort, e, decreases,
(iii) the probability that the good-type client gets rejected, R_G, and the overall rejection rate, R, increase, and
(iv) the entrepreneur's and the auditor's expected pay-offs, U^E and U^A, decrease.

Proof: See the Appendix.

One interpretation of litigation frictions is the auditor's cost of losing reputation in case of an audit failure. This expected cost can vary from client to client depending on media coverage or more generally the visibility of the client-firm or the industry.[20] The higher the visibility and media coverage of the potential client, the higher is the auditor's concern for potential reputation loss. Hence, our model predicts that clients with higher media coverage and visibility pay their auditors a higher audit fee and are more likely to be rejected, but if accepted, these clients obtain a higher quality audit.

Change in Legal Regime s

It is now straightforward to see how a shift in the strictness of the legal regime, s, affects the equilibrium outcome. On one hand, a higher s has beneficial effects because it increases the expected damage award, sD, the auditor has to pay investors in case of an audit failure. On the other hand, a larger s is detrimental because it increases the expected litigation friction, sF. These two effects work in opposite directions. For relatively low levels of s, the first effect dominates the second, and for relatively high levels of s, the reverse is true. This leads to the next proposition.

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[19] We have also considered a setting in which the outcome, X, and the damages for an audit failure, D, are increasing functions of the level of investment, I. In this case, an increase in I has the same effects as an increase in D, as presented in Proposition 4; that is, audit fee and evaluation effort increase, and rejection rates decrease (details regarding this scenario are available upon request.)

[20] For example, Johnstone and Bedard (2003) find that audit firms view publicly traded firms as relatively riskier than private firms, in part because of the scrutiny they receive.

Proposition 6: Let s_t denote the level of s that satisfies $(I - sD)\frac{da^*}{ds} - (1 - a^*)F = 0$. An increase in the strictness of the legal regime (increase in s) has the following equilibrium effects:

(i) the audit fee, W, increases,
(ii) the evaluation effort, e, increases for $s < s_t$ and decreases for $s > s_t$,
(iii) the probability that the good-type client gets rejected, R_G, and the overall rejection rate, R, decrease for $s < s_t$ and increase for $s > s_t$, and
(iv) the entrepreneur's and the auditor's expected pay-offs, U^E and U^A, increase for $s < s_t$ and decrease for $s > s_t$.

Proof: See the Appendix.

A shift to stronger legal regimes has an unequivocally positive effect on audit quality and audit fees given that a client is accepted, consistent with evidence reported by Seetharaman et al. (2002), Choi et al. (2008), and Venkataraman et al. (2008). However, the effects of the legal regime on client acceptance rates are not as straightforward. Proposition 6 implies a U-shaped relationship between the strictness of the legal regime and the probability of client rejection. Our model suggests relatively fewer client rejections in environments in which the legal regime is moderate, as compared to environments in which the legal regime is relatively strong (i.e., $s > s_t$) or relatively weak (i.e., $s < s_t$). The intuition behind this result is as follows. If the legal regime is relatively weak, then the induced audit quality is also relatively low, which results in suboptimal investment decisions. The anticipation of low audit quality therefore reduces the value of the entrepreneur's investment opportunity in the initial stage. As a consequence, the entrepreneur is unwilling to offer a high audit fee, which induces a low evaluation effort and hence a high rejection rate.[21] In environments in which the legal regime is strong, the audit quality will be relatively high, which leads to improved investment decisions. However, the litigation friction will also be relatively large, which makes it more expensive for the entrepreneur to attract the auditor. Due to this cost, the equilibrium rejection rate is larger in strong legal regimes than in moderate regimes.

Our model generates predictions with respect to changes in the legal liability environment caused by regulation such as the Sarbanes-Oxley Act of 2002 (SOX).[22] If a regulatory change results in an increase in the strength of the U.S. legal liability regime, s, then our model indicates that there are benefits and costs associated with such a change. The benefits are improved investment decisions due to higher levels of audit quality. The costs are the increase in litigation frictions that make hiring the auditor and hence implementing projects more expensive. Depending on whether the benefits exceed the costs (i.e., depending on whether $s < s_t$ or $s > s_t$), such a regulatory change either increases or decreases the probability of client rejection.

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[21] It is important to recall that our model only applies to potential clients who will be rejected if the auditor does not learn their type. If the legal regime is very weak, this set of potential clients may be small.

[22] The effects of SOX on auditors' litigation environment are unclear. See Asare at al. (2007) for a discussion of the legal implications of SOX for auditors.

A special case of our setting arises if plaintiffs' attorneys operate on a contingent fee basis, which is common practice in the U.S. In this case, the auditor's expected legal liability cost can be characterized by $s(Z\gamma + Z(1 - \gamma))$, where Z is the auditor's damage payment, γ is the fraction of Z that is recovered by investors, and $(1 - \gamma)$ is the fraction that is retained by the plaintiffs' attorneys. Since a change in damage payment Z is parallel to a change in s, an increase in Z has the same directional effects as an increase in s.

Expected Damage Payments Exceed Investor Losses

Throughout the analysis we focus on the case for which the expected damage payment to investors in case of an audit failure is lower than the investors' loss, $sD < I$. As discussed in Section II, we view this assumption as most reasonable. However, it is instructive to briefly discuss the case where $sD > I$.

For $sD > I$, the liability system induces the auditor to overinvest in audit quality relative to first-best (i.e., $a^* > a^f$). An increase in damages, D, will induce the auditor to choose an even higher audit effort, which reduces the total surplus generated in the case of a good-type client. A decrease in the total surplus reduces the value of the audit engagement, implying that the entrepreneur will choose an audit fee that induces less evaluation effort and a higher rejection rate. Hence, for $sD > I$, an increase in D reduces the pay-offs for both the auditor and the entrepreneur.

An increase in litigation frictions, F, continues to be detrimental to both players and leads to a lower evaluation effort and a higher client rejection rate. Since changes in D and F operate in the same direction, it is clear that an increase in the strength of the legal regime, s, leads to less evaluation effort, a higher rejection rate, and lower pay-offs for the auditor and the entrepreneur. Hence, if expected legal penalties are extreme (in the sense that they exceed investment losses), then increasing the strictness of the legal regime is unequivocally undesirable.

VI. THE ROLE OF PROJECT RISK

In this section we analyze the equilibrium effects of a change in project risk (i.e., change in θ) on the levels of the audit fee, evaluation effort, client rejection rate, and the players' pay-offs. Note that the auditor's expected litigation cost for a good-type client from an *ex ante* perspective is $(1 - \theta)(1 - a)s(D + F)$. Given this formulation, it is easy to see that an increase in project risk (i.e., an increase in $(1 - \theta)$) has effects similar to an increase in the strictness of the legal regime, s. That is, analogous to an increase in s, an increase in project risk has positive effects (since it induces higher audit quality) and negative effects (since it increases the litigation friction). In the following, we refer to these effects as "litigation risk" effects.

However, there is one important difference between a change in s and a change in project risk. An increase in project risk reduces the value of the entrepreneur's investment opportunity because the project will more likely fail. This loss in project value has important implications for the equilibrium outcome. To see this, recall that in our setting the audit fee serves as an incentive tool: the higher the fee offered by the entrepreneur, the higher is the auditor's incentive to evaluate the client (instead of rejecting the engagement uninformed) and the higher is the probability of client acceptance. If the expected value of the project declines due to a higher likelihood of failure, then the entrepreneur finds it less beneficial to provide the auditor with strong evaluation incentives and hence reduces the audit fee. Holding the litigation risk effect constant, the decline in project value therefore leads to a reduction in the equilibrium audit fee, the auditor's evaluation effort, and the pay-offs for both the auditor and the entrepreneur. We refer to this effect as the "project value" effect. Note that the "project value" effect is stronger if the outcome in case of project success, X, is larger.

Since a change in project risk is associated with both the litigation risk and the project value effects, it is difficult to provide clear-cut empirical predictions. However, for industries that produce high profits in the event of project success, the project value effect dominates the litigation risk effect. Suppose, for example, that $X > 2I + sF$. In this case, given the client is a good-type, the project has a positive expected value (for all $a \in (0,1)$), even when chances for success are only $\theta = 0.5$. This assumption seems to be consistent with risky industries for which only a small fraction of new start-ups succeed, but the start-ups that do succeed are highly profitable. In this situation, the following results hold.

Proposition 7: Suppose that $X \geq 2I + sF$. An increase in project risk, i.e., an increase in $(1 - \theta)$, has the following equilibrium effects:

(i) the audit quality, a, increases,
(ii) the audit fee, W, decreases,
(iii) the evaluation effort, e, decreases,
(iv) the probability that the good-type client gets rejected, R_G, and the overall rejection rate, R, increase,
(v) the entrepreneur's and the auditor's expected pay-offs, U^E and U^A, decrease.

Proof: See the Appendix.[23]

Our model predicts that firms that implement risky projects are more likely to be rejected by the auditor than firms that implement less risky projects. This result is consistent with survey and empirical evidence by Asare et al. (1994) and Johnstone and Bedard (2003). In addition, given that a client is accepted, our results suggest a positive link between project risk and audit quality, consistent with evidence in Bell et al. (2001). Finally, our model suggests that firms with higher project risk pay their auditors lower, not higher, audit fees. This result seems to be at odds with the empirical evidence on the link between litigation risk and audit fees (e.g., Beatty 1993; Simunic and Stein 1996). However, it is important to point out that this result is driven by our modeling feature that the audit fee serves as an incentive tool to motivate evaluation effort. For this reason, the fee offered to the auditor not only depends on the expected litigation and audit cost (as usually argued), but also on the value of the entrepreneur's investment opportunity. The lower the expected value of the investment opportunity, the less eager is the entrepreneur to induce a high evaluation effort by offering a large audit fee.

VII. CONCLUSION

There is substantial concern in the accounting and audit profession that excessive liability exposure adversely affects auditors' willingness to provide audit services to clients that are perceived to be high-risk. If true, auditors' propensity to reject clients is a problem because it reduces prospective clients' ability to access external capital markets to fund innovative new projects. Our goal is to shed light on these concerns.

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[23] The Appendix also provides the equilibrium effects of a change in $(1 - \theta)$ for the case where X does not satisfy the condition $X \geq 2I + sF$.

We find that audit quality and audit fees both increase with the auditor's expected litigation losses from audit failures. However, when considering the auditor's acceptance decision, we show that it is important to carefully identify the component of the litigation environment that is being investigated. We decompose the liability environment into three components: (1) the strictness of the legal regime, defined as the probability that the auditor is sued and found liable in case of an audit failure, (2) potential damage payments from the auditor to investors and (3) other litigation costs incurred by the auditor, labeled litigation frictions, such as attorneys' fees or loss of reputation. We show that, in equilibrium, an increase in the potential damage payment actually leads to a reduction in the client rejection rate. This effect arises because the resulting higher audit quality increases the value of the entrepreneur's investment opportunity, which makes it optimal for the entrepreneur to increase the audit fee by an amount that is larger than the increase in the auditor's expected damage payment. However, for this result to hold, it is crucial that damage payments be fully recovered by the investors. We show that an increase in litigation frictions leads to the opposite result—client rejection rates increase. Finally, since a shift in the strength of the legal regime affects both the expected damage payments to investors as well as litigation frictions, the relationship between the legal regime and rejection rates is non-monotonic. Specifically, we show that the relationship is U-shaped, which implies that for both weak and strong legal liability regimes, rejection rates are higher than those characterizing more moderate legal liability regimes.

The environment we consider is limited to settings in which the proposed investment project is relatively risky, such that without further information, the project will not be funded. For this reason, the auditor's propensity is to reject the client if uncertain about the client-type. Our information structure is a simplification of more realistic settings in which the auditor can acquire imperfect information about the prospective client. Despite these caveats, our model captures certain elements of auditor-client relationships that have attracted considerable attention in the professional and empirical literatures.

APPENDIX

Proof of Lemma 1

Substitute (7) into the entrepreneur's utility function (2) and let λ_1 denote the Lagrangian multiplier associated with (8). The Lagrangian of the problem is then:

$$\max_{W} LG = ep(\theta(X - I) - W - (1 - a)(1 - \theta)(I - sD)) + \lambda_1(p(W - (1 - a)(1 - \theta)L - k(a)) - c'(e)).$$

The necessary conditions for an optimal solution include:

$$\frac{\partial LG}{\partial W} = -ep + \lambda_1 p = 0, \tag{13}$$

$$\frac{\partial LG}{\partial e} = p(\theta(X - I) - W - (1 - a)(1 - \theta)(I - sD)) - \lambda_1 c''(e) = 0. \tag{14}$$

Substituting (14) into (13) and rearranging yields (10).

Proof of Proposition 4

From (6) and (11) it follows that:

$$\frac{da}{dD} = \frac{(1 - \theta)s}{k}, \tag{15}$$

$$\frac{de}{da} = \frac{p(1 - \theta)(I - sD)}{2c}, \tag{16}$$

which is positive for $sD < I$.

Evaluation Effort and Rejection Rate

Using (11) and (6) we have:

$$\frac{de}{dD} = \frac{p(1 - \theta)(I - sD)}{2c}\frac{da}{dD} = \frac{de}{da}\frac{da}{dD}, \tag{17}$$

which is positive since $\frac{de}{da} > 0$ and $\frac{da}{dD} > 0$. From this it follows that $\frac{dR_G}{dD} < 0$ and $\frac{dR}{dD} < 0$.

Audit Fee

From (10) it follows that:

$$W^* = -e^*\frac{c}{p} + \theta(X - I) - (1 - a^*)(1 - \theta)(I - sD). \tag{18}$$

The first derivative of (18) with respect to D is:

$$\frac{dW^*}{dD} = -\frac{de^*}{dD}\frac{c}{p} + (1 - \theta)\frac{da^*}{dD}(I - sD) + (1 + a^*)(1 - \theta)s. \tag{19}$$

Substituting (17) into (19) and rearranging gives:

$$\frac{dW^*}{dD} = 0.5(1 - \theta)(I - sD)\frac{da^*}{dD} + (1 - a^*)(1 - \theta)s,$$

which is positive since we assumed that $sD < I$.

The Auditor's Utility

The auditor's *ex ante* utility is given by:

$$U^A = ep[W - (1 - a)(1 - \theta)L - 0.5ka^2] - 0.5ce^2. \tag{20}$$

Substituting (8) into (20) gives:

$$U^A = 0.5ce^2. \tag{21}$$

The first derivative of (21) with respect to D is:

$$\frac{dU^A}{dD} = ec\frac{de}{dD},$$

which is positive since $\frac{de}{dD} > 0$.

The Entrepreneur's Utility

Substituting the investors' participation constraint into the entrepreneur's utility function (2) gives:

$$U^E = ep(\theta(X - I) - W - (1 - a)(1 - \theta)(I - sD)). \tag{22}$$

Substituting (18) into (22) and rearranging yields:

$$U^E = ce^2. \tag{23}$$

The first derivative of (23) with respect to D is:

$$\frac{dU^E}{dD} = 2ce\frac{de}{dD},$$

which is positive since $\frac{de}{dD} > 0$.

Proof of Proposition 5

From (6) it follows that:

$$\frac{da}{dF} = \frac{(1 - \theta)s}{k} > 0. \tag{24}$$

From (11) it follows that:

$$\frac{de}{da} = \frac{p(1 - \theta)(I - sD)}{2c} > 0. \tag{25}$$

Evaluation Effort and Rejection Rate

Using (11) and (6) we have:

$$\frac{de}{dF} = p(1 - \theta)\frac{(I - sD)\frac{da}{dF} - (1 - a)s}{2c} = \frac{de}{da}\frac{da}{dF} - \frac{p(1 - \theta)(1 - a)s}{2c}. \tag{26}$$

Note that $a = \frac{(1 - \theta)s(D + F)}{k}$ and $a^f = \frac{(1 - \theta)(I + sF)}{k}$. Substituting $\frac{da}{dF} = \frac{(1 - \theta)s}{k}$ into (26) yields:

$$\frac{de}{dF} = -\frac{p(1-\theta)}{2c}s(1-a^f),$$

which is negative. For $\frac{de}{dF} < 0$, it follows that $\frac{dR_G}{dF} > 0$ and $\frac{dR}{dF} > 0$.

Audit Fee

The first derivative of (18) with respect to F is:

$$\frac{dW}{dF} = -\frac{de}{dF}\frac{c}{p} + (1-\theta)\frac{da}{dF}(I - sD),$$

which is positive since $\frac{de}{dF} < 0$, $\frac{da}{dF} > 0$, and we assumed that $sD < I$.

The Auditor's Utility

The first derivative of (21) with respect to F is:

$$\frac{dU^A}{dF} = ec\frac{de}{dF},$$

which is negative since $\frac{de}{dF} < 0$.

The Entrepreneur's Utility

The first derivative of (23) with respect to F is:

$$\frac{dU^E}{dF} = 2e\frac{de}{dF}c,$$

which is negative since $\frac{de}{dF} < 0$.

Proof of Proposition 6

From (6) and (11) it follows that:

$$\frac{da}{ds} = \frac{(1-\theta)(D+F)}{k} > 0,$$

$$\frac{de}{da} = \frac{p(1-\theta)(I-sD)}{2c} > 0.$$

Evaluation Effort and Rejection Rate

Using (11) we have:

$$\frac{de}{ds} = p\frac{(1-\theta)(I-sD)\frac{da}{ds}}{2c} - p\frac{(1-a)(1-\theta)F}{2c}. \tag{27}$$

Note that the second derivative is:

$$\frac{d^2e}{ds^2} = -\frac{p}{2c}(1 - \theta)[D - F]\frac{da}{ds},$$

which is negative since we assumed that $D > F$. This implies that there exists a threshold s_t that satisfies $(I - sD)\frac{da^*}{ds} - (1 - a^*)F = 0$ such that for $s < s_t$ it holds that $\frac{de}{ds} > 0$ and for $s > s_t$ it holds that $\frac{de}{ds} < 0$. For $\frac{de}{ds} > 0$, we have $\frac{dR_G}{ds} < 0$, $\frac{dR}{ds} < 0$ and for $\frac{de}{ds} < 0$, we have $\frac{dR_G}{ds} > 0$, $\frac{dR}{ds} > 0$.

Audit Fee

The first derivative of (18) with respect to s is:

$$\frac{dW}{ds} = -\frac{de}{ds}\frac{c}{p} + [1 - \theta]\frac{da}{ds}(I - sD) + (1 - a)(1 - \theta)D. \tag{28}$$

Substituting (27) into (28) and rearranging gives:

$$\frac{dW}{ds} = 0.5(1 - a)(1 - \theta)F + 0.5[1 - \theta]\frac{da}{ds}(I - sD) + (1 - a)(1 - \theta)D,$$

which is positive since we assumed that $sD < I$.

The Auditor's Utility

The first derivative of (21) with respect to s is:

$$\frac{dU^A}{ds} = ec\frac{de}{ds},$$

which is positive for $\frac{de}{ds} > 0$ and negative for $\frac{de}{ds} < 0$.

The Entrepreneur's Utility

The first derivative of (23) with respect to s is:

$$\frac{dU^E}{ds} = 2ec\frac{de}{ds},$$

which is positive for $\frac{de}{ds} > 0$ and negative for $\frac{de}{ds} < 0$.

Proof of Proposition 7

Evaluation Effort and Rejection Rate

From (11) it follows that:

$$\frac{de}{d\theta} = p\,\frac{X - Ia + (1-\theta)\dfrac{da}{d\theta}(I - sD) + (1-a)sF}{2c}. \tag{29}$$

Substituting $a = \dfrac{(1-\theta)s(D+F)}{k}$ and $\dfrac{da}{d\theta} = -\dfrac{L}{k}$ into (29) and rearranging yields:

$$\frac{de}{d\theta} = p\,\frac{X - 2Ia + asD + (1-a)sF}{2c}, \tag{30}$$

which is positive for $X - 2I - sF > 0$. For $\dfrac{de}{d\theta} > 0$, it follows that $\dfrac{dR_G}{d\theta} < 0$ and $\dfrac{dR}{d\theta} < 0$.

Audit Fee

From (18) it follows that:

$$\frac{dW}{d\theta} = -\frac{de}{d\theta}\frac{c}{p} + X - Ia - (1-a)sD + (1-\theta)(I - sD)\frac{da}{d\theta}. \tag{31}$$

Substituting (30), $a = \dfrac{(1-\theta)s(D+F)}{k}$, and $\dfrac{da}{d\theta} = -\dfrac{L}{k}$ into (31) and rearranging yields:

$$\frac{dW}{d\theta} = 0.5X - Ia - (1-a)sD - 0.5(1-a)sF + 0.5asD,$$

which is positive for $0.5X - I - 0.5sF > 0$ since we assumed that $sD < I$.

The Auditor's Utility

The first derivative of (21) with respect to θ is:

$$\frac{dU^A}{d\theta} = ec\,\frac{de}{d\theta},$$

which is positive for $\dfrac{de}{d\theta} > 0$ and negative for $\dfrac{de}{d\theta} < 0$.

The Entrepreneur's Utility

The first derivative of (23) with respect to θ is:

$$\frac{dU^E}{d\theta} = 2e\,\frac{de}{d\theta}\,c,$$

which is positive for $\dfrac{de}{d\theta} > 0$ and negative for $\dfrac{de}{d\theta} < 0$.

This analysis leads to the next proposition, which is more general than the one provided in Section VI.

Proposition 8: *Let* X^A and X^B denote the thresholds that satisfy:

$$X^A - Ia^* + (1 - \theta)(I - sD)\frac{da^*}{d\theta} + (1 - a^*)sF = 0$$

and:

$$X^B - Ia^* + (1 - \theta)(I - sD)\frac{da^*}{d\theta} + (1 - a^*)sF - 2(1 - a^*)(sD + sF) = 0,$$

respectively.

An increase in the project risk (i.e., a reduction in θ) has the following equilibrium effects:

(i) the audit fee, W, decreases for $X > X^B$ and increases for $X < X^B$;
(ii) the evaluation effort, e, decreases for $X > X^A$ and increases for $X < X^A$;
(iii) the probability that the good-type client gets rejected, R_G, and the overall rejection rate, R, increase for $X > X^A$ and decrease for $X < X^A$; and
(iv) the entrepreneur's and the auditor's expected pay-offs, U^E and U^A, decrease for $X > X^A$ and increase for $X < X^A$.

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Accounting Irregularities and Executive Turnover in Founder-Managed Firms

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ABSTRACT: This study tests the hypothesis that founder CEOs are less likely to be fired than non-founder CEOs when accounting irregularities are disclosed. We also examine whether CFOs are more likely to shoulder the blame when the CEO is a founder. Using a sample of 96 newly public firms with accounting irregularities, and a control sample of similar newly public firms, we document that the probability of CEO (CFO) turnover in the wake of an accounting irregularity is lower (higher) when the firm's CEO is also a founder. The difference in CEO turnover rates is dramatic, with non-founder CEOs turning over at a rate of 49 percent, as compared to only 29 percent for founder CEOs. Our overall findings are consistent with the notion that the board's response to irregularities differs when the CEO is a founder.

Keywords: *entrenchment; turnover; founders; restatements; IPO.*

Data Availability: *The data used in this study are available from public sources identified in the text. The classification of the GAO sample is also available from the authors upon request.*

JEL Classifications: *G32, G14.*

I. INTRODUCTION

In recent accounting scandals involving the backdating of executive stock options and subsequent restatement of firm financials, the outcomes for numerous chief executive officers (CEOs), chief financial officers (CFOs), and other executives involved termination of employment. Additionally, a number of these executives faced further

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consequences, including criminal prosecution.[1] However, in the case involving the backdating of executive stock options at Apple Computers, a special committee of the board of directors absolved Steven Jobs, Apple's founder and CEO, of any wrongdoing. Instead of holding the CEO accountable, Apple's special committee placed responsibility for the accounting irregularity on two former officers of the company. In response to the findings and actions of Apple's special committee, outsiders voiced criticism. Lynn Turner, former chief accountant at the Securities and Exchange Commission (SEC), commented, "It appears as if Jobs is playing the role of a monkey: See no evil, hear no evil, speak no evil. If he truly were fulfilling his role as CEO, it is highly questionable as to why he didn't know about such poor management and oversight of the option-granting process."[2]

The Apple Computers case is only anecdotal, and a number of factors could explain why Steve Jobs was not fired in the wake of the backdating scandal at Apple.[3] But this example does raise questions of whether founder CEOs, such as Steve Jobs, are more likely to avoid culpability when accounting irregularities occur than are non-founder or "hired" CEOs. The purpose of this study is to test whether the case of Steve Jobs and Apple Computers is an aberration or the norm.[4] Specifically, we investigate whether founder CEOs are less likely than non-founder CEOs to be fired when accounting irregularities occur. Additionally, we test whether CFOs are more likely to "shoulder the blame" as the designated "scapegoat" (i.e., be fired) when accounting irregularities occur in founder CEO firms.

Prior research documents that CEO and CFO turnover is unusually high when accounting irregularities are reported. Hennes et al. (2008) report that in roughly 80 percent of the cases in which an irregularity is disclosed, either the CEO or CFO is terminated. The authors argue that the firing of the CEO and/or CFO is typically necessary for at least two reasons. First, auditors must rely on the judgment of management in order to express an opinion on the financial statements. If the auditors believe that members of management were involved in an irregularity, and if those involved are not fired, then the auditors will be obliged to resign from the engagement. Second, the SEC considers the firm's response to an irregularity in deciding (1) whether to undertake an enforcement action and (2) the extent of potential fines and penalties. Among the factors considered is whether the board terminates those responsible for the irregularity.

Although terminating the typical CEO or CFO is costly for firms involved in an accounting irregularly, we argue in this study that the costs of terminating/replacing CEOs who are also *founders* are considerably higher than for non-founder CEOs, thereby causing CEO turnover rates to be much lower for these firms. Palia et al. (2008) argue that founders are harder to remove because of their asset-specific capital. Entrepreneurs identify investment opportunities that match their own human capital and, subsequently, these individuals continue to develop their human capital to increase the value of firm assets. Although the synergy between the founder CEO's human capital and the firm's assets tend to increase the firm's value, the implication is that subsequently replacing the founder CEO would cause a decline in firm value (since the next best CEO would not have an equivalent set of firm-specific skills). Consequently, CEO replacement costs are higher in firms where the CEO is also the founder. The higher replacement costs for founder CEOs imply an equilibrium in which, *ceteris paribus*, founder CEOs are less likely to be fired in the wake of an accounting irregularity.

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[1] For example, Gregory Reyes, the former CEO of Brocade Communication Systems, was convicted on August 11, 2007 on ten counts of illegally backdating stock options. Additionally, William McGuire, the former CEO of UnitedHealth Group Inc., agreed on December 6, 2007 to repay $468 million as a partial settlement with the SEC.

[2] This quote appeared in *The New York Times* on December 30, 2006 in an article by Floyd Norris entitled "The $2.8 Billion Mistake by Steve Jobs."

[3] One plausible explanation is that Steve Jobs was unaware of the options backdating practice at Apple Computers.

[4] On the day that Apple Computers announced that CEO Steve Jobs was cleared of any wrongdoing, Apple's stock price increased 4.9 percent. This reaction suggests that investors viewed this announcement favorably.

At the same time, as discussed above, it is typically necessary for the firm to fire someone in the wake of an accounting irregularity. Given the relatively high costs of firing founder CEOs, coupled with the high costs of not firing anyone, we hypothesize that, when possible, boards will attempt to shield the founder CEO from blame—and designate the CFO as the "scapegoat"—when accounting irregularities occur.

To test our hypothesis, we obtain a sample of restatements from the Government Accountability Office (GAO) database, which spans the time period 1997–2006, and we focus on restatements related to accounting irregularities. From this sample of 666 irregularities, we further reduce our sample to irregularities that occurred within eight years of the firm's initial public offering (IPO). This requirement is likely to increase the probability that (1) a higher proportion of founders will be in our sample and (2) the founder's manager-specific capital is valuable to the survival and success of the firm.[5] After excluding firms with missing Compustat and CRSP data, our final sample contains 96 firms with accounting irregularities. We then select a control sample of similar newly public firms, matched within industry by nearest IPO date, resulting in a total sample of 192 observations.

We find that the turnover rate for founder CEOs increases from 10.3 percent to 29.2 percent (an increase of 18.9 percent) when an accounting irregularity is disclosed. Although the turnover rate in firms with founder CEOs increases significantly when an irregularity is disclosed, the increase is substantially less than for non-founder CEOs. The turnover rate for non-founder CEOs increases from 10.5 percent to 48.6 percent (an increase of 38.1 percent).[6] Consistent with our predictions, the evidence suggests the impact of an irregularity on the probability of CEO turnover is roughly twice as high when the CEO is not the founder (38.1 percent versus 18.9 percent). We also find that in irregularity firms, the turnover rate for CFOs actually increases when the CEO is a founder. This evidence is consistent with company boards being inclined to designate the CFO as the "scapegoat," particularly in the case of founder CEOs.[7]

In addition to our main tests, we consider a number of alternative hypotheses and conduct a variety of robustness tests. First, we test whether the influence of founder CEOs is caused by their ownership in the firm, as opposed to founder-specific characteristics (e.g., manager-specific investments). Second, we test whether the results of our findings are driven by the fact that founder CEOs have less accounting expertise than non-founder CEOs. If founder CEOs have less accounting expertise, then they may be less involved in the accounting decisions, thereby avoiding culpability for the irregularity. Third, we consider whether poor governance arising from a smaller proportion of outside board members in founder CEO firms is the cause of the lower turnover rates for founder CEOs. Finally, we consider whether our results are driven by unusual turnover rates in our control sample, rather than the turnover practices within the irregularity sample. Our results hold after empirically addressing these alternative explanations.

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[5] Another benefit of limiting the sample in this manner is that the costs of hand-collection are reduced. One consequence of this approach is that the generalizability of our results is limited, and our results only apply to founders of newly public firms.

[6] The unconditional turnover rates for our control samples (10.3 percent and 10.5 percent) are in line with past research (e.g., Fee and Hadlock 2004).

[7] We find that blame shifting from the CEO toward the CFO is *not* more frequent following an accounting irregularity, unless the CEO is the founder.

Our findings contribute to an understanding of the relation between executive turnover and accounting irregularities. Although intuition would suggest that turnover would be lower for CEOs of founder-managed firms, the magnitude of the impact is quite striking. This has implications for researchers and regulators. Future research that attempts to better understand executive turnover, particularly around restatements, should incorporate proper controls for the influence of founder CEOs. Without such controls, such tests will potentially suffer from a correlated omitted variables problem.

For regulators, our findings also suggest that more research is warranted into the nature and level of independence involved in the investigatory process around accounting irregularities. The vast majority of the firms in our sample announced an internal investigation into the alleged accounting irregularities. Nonetheless, evidence suggests that these board-initiated investigations may lack objectivity when the CEO in question is also the founder of the firm.

We urge some caution when interpreting our results, as our empirical tests cannot definitively ascertain whether the founder CEO is actually "innocent" of the accounting irregularity. Hence, one alternate explanation for our findings is that the CEOs (and CFOs) of founder firms are less (more) likely to be fired because they are actually innocent (guilty). However, we conduct tests that demonstrate that the fired-CFOs of founder CEO firms are likely "scapegoats." Further tests also reveal that even in cases in which the founder CEOs sold stock prior to the restatement, thus benefiting from misstated financials, the founder CEOs were still less likely to be fired. Taken together with our robustness tests, these tests are more consistent with our hypotheses and less consistent with the alternate explanation.

Section II discusses prior literature and develops our hypotheses. Section III describes our model and sample selection procedure. Section IV reports results, and Section V concludes.

II. PRIOR LITERATURE AND HYPOTHESIS DEVELOPMENT

It is well documented that accounting irregularities lead to a significant decline in firm value, if not bankruptcy. For example, Hennes et al. (2008) report a mean (median) market decline of −13.64 (−19.14) percent in the seven days surrounding the announcement of an accounting irregularity. The large decline in market value is due to several factors, including increases in the firm's cost of capital due to reduced credibility of financial disclosure (see Palmrose et al. 2004; Hribar and Jenkins 2006), as well as increases in the expected cost of litigation and regulatory actions, monitoring, and future regulatory scrutiny (Palmrose et al. 2004). When faced with an accounting irregularity, boards consider a variety of actions to mitigate the associated costs, one of which is firing key executives.

Executive Turnover around Accounting Irregularities

Prior research has examined the relation between accounting restatements and executive turnover (Beneish 1999; Desai et al. 2006; Land 2006; Arthaud-Day et al. 2006; Jayaraman et al. 2004; Karpoff et al. 2008; Hennes et al. 2008). Although the turnover rates reported in these studies tend to vary due to different research design choices (e.g., various turnover windows, types of executives, and types of restatements), the overall evidence is consistent with higher turnover rates around the disclosure of a restatement. For example, Hennes et al. (2008) examine executive turnover within the time period beginning six months prior to, and ending six months after, the announcement of a restatement related to an accounting irregularity. They find that 49 percent of CEOs and 63 percent of CFOs are replaced within this window, while higher turnover rates of 54 percent for CEOs and 70 percent of CFOs occur when the sample is restricted to parent-level restatements.

Several reasons account for the high turnover rates. First, firing senior executives is a way to restore financial reporting credibility (Farber 2005; Desai et al. 2006). Second, auditors will be forced to resign if the senior managers involved in the scandal are not fired (Hennes et al. 2008). Third, the SEC is less likely to take an enforcement action, and may even reduce the penalties, if those involved in the irregularity are fired. Generally, the most direct way to mitigate problems associated with intentional financial misstatements is to terminate the executives that are responsible for the wrongdoing (e.g., the CEO and CFO).

Executive Turnover When the CEO is Also the Founder

The CEO replacement decision becomes more complicated when the CEO is also the founder because these CEOs are generally more costly to replace. Palia et al. (2008) state that founders may be difficult to remove from office because of their asset-specific capital, and this situation leads to entrenchment. Founders initially make value-maximizing investments that also happen to be manager-specific. Founder CEO entrenchment occurs as a natural outcome of making value-maximizing investment decisions in setting up, building, and founding a firm. As a consequence, replacing the CEO is costly, since the next best CEO will not operate the firm's assets as efficiently as the founder CEO. Following the assumptions in Morck et al. (1988) and Palia et al. (2008), we assume that founder CEOs are "valuable" and/or entrenched.

Although prior studies describe the process of becoming entrenched as one in which managers make manager-specific investments that are *not* necessarily value-maximizing to the firm (e.g., Shleifer and Vishny 1989), the presence of entrenched founder CEOs does not necessarily reduce firm value.[8] In fact, even though founder CEOs appear to be entrenched, prior research suggests that these firms outperform those managed by non-founder CEOs (Morck et al. 1988; Adams et al. 2005; Palia et al. 2008; Villalonga and Amit 2006). There are two potential explanations for these findings. First, Palia et al. (2008) argue that founder CEOs tend to work harder than non-founder CEOs. Because of their manager-specific investments, founder CEOs work more efficiently and thus exert more effort for a given level of incentives. Second, Villalonga and Amit (2006) argue that, in the case of founder CEOs, the reduction in agency costs arising from the typical separation of ownership and control, as discussed in Jensen and Meckling (1976), outweighs any increase in agency costs that arise from founder CEO entrenchment.[9] In summary, founder CEOs are entrenched, in the sense that there will be a reduction in firm value if they have to be replaced. Therefore, boards have incentives to retain them even in the face of an accounting irregularity.

One obvious and open issue is *how* CEOs manage to avoid being fired in the wake of an accounting irregularity. Interestingly, the SEC encourages firms to undertake their own internal, independent investigation into the exact nature of the accounting irregularity. The SEC also indicates that, in cases where the board of directors takes corrective actions, including firing those employees responsible for the accounting irregularity, the penalties and punishment against the firm will be less severe.[10] To the extent that the board's conclusion steers the blame away from the CEO and influences subsequent SEC investigations, the board can effectively reduce the probability that the CEO will be the target of the SEC enforcement action.[11]

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[8] Consistent with the entrenchment hypothesis, Denis et al. (1997) report that one proxy for entrenchment, managerial ownership, decreases the relation between performance and CEO turnover. The authors argue that increases in managerial ownership of the firm may make it more difficult and costly to remove a manager who is performing poorly.

[9] Fee and Hadlock (2004) document that there is no negative relation between CEO entrenchment and non-CEO turnover. The authors state, "It appears that entrenched or powerful CEOs are not particularly soft or hard on their immediate subordinates in terms of the likelihood of dismissal."

Consider, for example, the case of GMH Community Trust. In this case, the CFO of GMH, Bradley W. Harris, wrote a letter in 2006 to the firm's board of directors expressing concern about the pressure he received to manage the firm's earnings. The following was disclosed in the firm's 10-K for the period December 31, 2005, Item 9A, Controls and Procedures: "During the first quarter of 2006, the Audit Committee of our Board of Trustees initiated an investigation promptly following receipt of a letter from Bradley W. Harris, who at the time was our Chief Financial Officer, alleging, among other things, a 'tone at the top' problem within management, and raising concerns regarding various accounting methodologies that were being considered by management in connection with certain transactions that occurred in the fourth quarter of 2005."[12] Although GMH's audit committee concluded that no wrongdoing had occurred, the firm decided to restate its earnings and fire its CFO on the same day.[13] This anecdote suggests the possibility that CEOs that are aware of and/or potentially responsible for accounting irregularities may successfully use their influence to (1) avoid getting fired and (2) convince the board to fire the CFO.

In summary, both the incentives and the means exist for firms to shield the CEO from responsibility for accounting irregularities. Although a CEO who is "caught red-handed" may not be able to avoid being fired, it is likely that many cases are somewhat ambiguous. In these situations, we expect the board to be more likely to protect the CEO if this individual is also the founder. This leads to the following hypothesis:

> **H1:** The probability of CEO turnover around the announcement of an accounting irregularity is lower for founder CEO firms than for non-founder CEO firms.

Given prior evidence in Hennes et al. (2008) that roughly 80 percent of accounting irregularities lead to the termination of either the CEO or the CFO, there is clearly tremendous pressure for boards to hold at least one of the two senior-level executives responsible. If H1 is true, then it follows that CFOs are more likely to be fired when the CEO is also the founder. Although shareholders likely benefit from the retention of founder CEOs, it is still necessary for the firm to restore their financial statement credibility and to minimize regulatory costs. Consequently, since the CEO is more likely to be retained when s/he is a founder of the firm, we expect that in these cases, there is an increased probability that the CFO will be terminated. In other words, the CFO is blamed in order for the CEO to be retained. The next hypothesis that we derive from this reasoning is as follows:

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[10] Text of the SEC's rulings suggest that the firm can substantially reduce the likelihood of an SEC enforcement action altogether by proactively contacting the SEC to communicate about the case, promptly initiating an independent investigation of the irregularity, and assigning blame and punishment to any involved employees. See AAER No. 1470 (http://www.sec.gov/litigation/investreport/34-44969.htm) for an example of the SEC's recommendations.

[11] In a related study, Dechow et al. (1996) examine firms that are subject to enforcement actions from the SEC. The authors find that restating firms are more likely than control firms to have a founder CEO, as well as higher percentage of inside directors.

[12] We interpret "tone at the top" to be a subtle reference to pressure from the firm's CEO.

[13] We traced the subsequent employment record of Bradley W. Harris after his firing from GMH. Harris was last listed as working for GMH in March 2006. On September 5, 2006, Harris joined International Coal Group (ICO) as CFO and vice-president. As of ICO's most recent 10-Q statement dated November 7, 2008, Harris, at age 48, is still listed as CFO.

H2: The probability of CFO turnover around the announcement of an accounting irregularity is higher for founder CEO firms than for non-founder CEO firms.

In founder CEO firms, the lower CEO turnover rates (H1) and higher CFO turnover rates (H2) are necessary—but not sufficient—conditions for scapegoating of the CFO. That is, if CFO scapegoating occurs, it must be the case that CEO (CFO) turnover is lower (higher) for founder CEO firms; however, it is possible for both of these conditions to hold even in the absence of scapegoating. For example, the increased likelihood of CFO turnover in irregularity firms with founder CEOs may reflect cases in which *both* the founder CEO and CFO were fired; thus empirically, it would be possible for both H1 and H2 to hold, and yet *no* blame is shifted (e.g., *both* executives are fired, and/or *both* executives are retained). A scapegoating scenario in which the CEO is retained and the CFO is fired is described in the following hypothesis:

H3: The probability that the CEO is retained while the CFO departs when an irregularity occurs is higher for founder CEO firms than for non-founder CEO firms.

III. MODELS AND SAMPLE

Models

To examine how founder CEOs influence CEO and CFO turnover rates, we estimate the following logit models, define the following variables, and discuss our predictions for each model:

$$\begin{aligned} \mathit{CEO\ TURNOVER} = {} & \beta_0 + \beta_1 \mathit{IRREGULARITY} + \beta_2 \mathit{FOUNDER\text{-}CEO} \\ & + \beta_3 \mathit{IRREGULARITY} * \mathit{FOUNDER\text{-}CEO} + \beta_4 \mathit{ANNUAL} \\ & + \beta_5 \mathit{SUBSIDIARY} + \beta_6 \mathit{Log}(\mathit{Sales} + 1) \\ & + \beta_7 \mathit{BHAR}_{[-250,-8]} + \beta_8 \mathit{PERFORMANCE}_{t-1} + \varepsilon \qquad (1) \end{aligned}$$

where:

CEO (CFO) TURNOVER = 1 if the CEO (CFO) was replaced within 13 months of the restatement announcement (six months prior to six months after the announcement), and 0 otherwise;
CFO_NOCEO_TURNOVER = 1 if the CEO is retained but the CFO departs within 13 months of the restatement announcement, and 0 otherwise;
IRREGULARITY = 1 if the firm has an accounting irregularity and 0 if the firm is a control firm;
FOUNDER-CEO = 1 if the firm's CEO prior to the restatement is described as a founder of the firm, and 0 otherwise;
ANNUAL = 1 if the annual financials are restated, and 0 if the quarterly financials are restated;
SUBSIDIARY = 1 if the irregularity occurred at a subsidiary (as opposed to at the parent level), and 0 otherwise;
Log(Sales + 1) = log (sales + 1) = log(Compustat #12 + 1);

$BHAR_{[t-250,t-8]}$ = buy-and-hold return for the firm − buy-and-hold return for the S&P 500 Index = [(price$_{t-8}$/price$_{t-250}$) − 1] − [(S&P500 Index$_{t-8}$/S&P500 Index$_{t-250}$) − 1]; abnormal stock performance from day $t-250$ to day $t-8$, where day $t-250$ is 250 trading days prior to the restatement announcement and day $t-8$ is eight trading days prior to the restatement announcement; daily price information is obtained from CRSP data item "prc"; S&P500 Index is obtained from CRSP data item "spindx"; and

PERFORMANCE = either ROA_{t-1}, income in the year prior to the restatement divided by beginning of year assets (Compustat #18/Compustat #6), or $CASHOP_{t-1}$, cash from operations in the year prior to the restatement divided by beginning of the year assets (Compustat #308/Compustat #6), depending on model specification.

To identify whether CEO turnover and CFO turnover occurs, we read through the firm's 10-K, 10-Q, 8-K, and Proxy Statements filings from the SEC's EDGAR database. We begin with all filings dated six months prior to the restatement announcement, and we search for the terms "chief" (to identify events involving either the chief executive officer or chief financial officer), "CEO," and/or "CFO."[14] We code the firm as having executive turnover if any of the following events has taken place: (1) the firm makes an official announcement of the executive's departure, (2) the firm makes an official announcement of the hiring of the replacement, or (3) a new executive has begun to certify the firm's financial statements in an official capacity.[15] We continue this process of ascertaining executive turnover until we reach firm filings dated six months after the restatement announcement, as this marks the end of the event window. Following Hennes et al. (2008) we limit our turnover window to 13 months (six months before to six months after the announcement).[16]

Beginning with model (1), based on prior research, we expect the coefficient on *IRREGULARITY* (β_1) to be positive (+) and significant. Consistent with Parrino (1997) and Morck et al. (1988) we predict the coefficient on *FOUNDER-CEO* (β_2) to be negative (−)

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[14] If the irregularity was disclosed in the middle of the month (e.g., June 15, 2005), we assume the turnover window begins six months prior to the beginning of the month of disclosure (e.g., January 1, 2005).

[15] We initially chose *not* to try to distinguish between forced and unforced turnover because prior research suggests that voluntary turnover rates are rare relative to forced turnover in our context. For example, Hennes et al. (2008) report that virtually all of the turnover occurring in their irregularity sample would be considered forced using the Parrino (1997) definition of "forced." For this reason, we would expect only a minor loss in power by not trying to distinguish forced from unforced turnover. Nonetheless, to address the potential impact of voluntary turnover, we further investigate the circumstances surrounding CEO turnovers for both samples (96 irregularities and 96 control firms). Within the samples, CEO turnover occurred in 42 irregularity firms and 10 control firms, and we found only three cases of potential voluntary departure (one in the irregularity sample and two in the control sample). For example, in one of these voluntary turnover cases, the executive was joining a larger, public corporation that was named in the press release. Moreover, if we re-classify these three cases as non-turnover events, our findings and inferences are unaffected.

[16] We use a relatively short window (six months before to six months after) to capture turnover related to the irregularity event. Hennes et al. (2008) show that most of the turnover related to irregularities occurs within this period. Although expanding this window will likely add "noise" to our measures, we acknowledge that (1) there is a likelihood that executive turnover related to the restatement may continue well after the six-month window following the irregularity and (2) there is no clear consensus on the appropriate window for such events. Consequently, we have conducted robustness tests over a two-year window (one year before to one year after), and our findings and inferences are unaffected by the choice of window.

and significant.[17] Hypothesis 1 predicts that the coefficient on *IRREGULARITY* * *FOUNDER-CEO* (β_3) in model (1) is negative (−).

$$\begin{aligned} CFO\ TURNOVER = {} & \beta_0 + \beta_1 IRREGULARITY + \beta_2 FOUNDER\text{-}CEO \\ & + \beta_3 IRREGULARITY * FOUNDER\text{-}CEO + \beta_4 ANNUAL \\ & + \beta_5 SUBSIDIARY + \beta_6 Log(Sales + 1) \\ & + \beta_7 BHAR_{[-250,-8]} + \beta_8 PERFORMANCE_{t-1} + \varepsilon. \end{aligned} \tag{2}$$

Turning to model (2), we expect the coefficient on *IRREGULARITY* (β_1) to be positive (+) and significant. We make no prediction on the coefficient on *FOUNDER-CEO* (β_2) because we have no reason to expect average CFO turnover to be any different for founder CEO firms. We also predict a positive (+) coefficient for β_3 on the interaction term *IRREGULARITY* * *FOUNDER-CEO*:

$$\begin{aligned} CFO\_NOCEO\_TURNOVER = {} & \beta_0 + \beta_1 IRREGULARITY + \beta_2 FOUNDER\text{-}CEO \\ & + \beta_3 IRREGULARITY * FOUNDER\text{-}CEO \\ & + \beta_4 ANNUAL + \beta_5 SUBSIDIARY \\ & + \beta_6 Log(Sales + 1) + \beta_7 BHAR_{[-250,-8]} \\ & + \beta_8 PERFORMANCE_{t-1} + \varepsilon. \end{aligned} \tag{3}$$

Model (3) effectively is a joint test of H1 and H2, which we refer to as H3 for convenience. Here, we predict that the coefficient on the interaction term *IRREGULARITY* * *FOUNDER-CEO* (β_3) is positive (+). This means that if the CEO is a founder and there is an accounting irregularity, there is a high likelihood that the CEO is retained, while the CFO departs. We do not make a prediction on *IRREGULARITY*, nor do we make a prediction on *FOUNDER-CEO*.

Sample Selection

We begin our sample selection procedures with restatements obtained from two GAO databases (spanning the time periods 1997–2002 and 2002–2006), which contain 2,704 observations combined (see Table 1). Next, we limit the sample to accounting irregularities, according to the classification scheme developed in Hennes et al. (2008). A restatement is classified as an accounting irregularity, as opposed to an accounting error, if any one of the following conditions is met: (1) the firm uses variants of the words "irregularity" or "fraud" in describing the misstatement in a press release, (2) the firm announces an independent internal investigation, or (3) the firm announces an SEC or Department of Justice investigation, which may involve the Attorney General. The selection criteria, which are designed to identify accounting irregularities among the total restatements, reduce the sample to 666 observations.

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[17] Parrino (1997) and Morck et al. (1988) find that if the CEO is a member of the founding family, the probability of CEO turnover is lower. The authors define their founder indicator variable as equal to 1 if the CEO is *related* to the founding family but not necessarily the founder. The coefficient on the founder variable is not statistically significant in several of Parrino's (1997) model specifications, though he conjectures that this is likely due to a lack of power. However, we restrict our definition of founder to those individuals that directly founded the firm.

TABLE 1
Sample Selection

Number of restatement observations identified from the Government Accountability Office (GAO) database	2,704
Number of restatement observations that are related to accounting irregularities	666
Number of restatement firms where an initial public offering (IPO) date could be determined	309
Number of firms in which the restatement occurred within eight years of the fiscal year of IPO	165
Total available firm observations	165
Less firms with missing Compustat industry information	−38
Less firms with missing prospectus information from Form 424 filings on the SEC's EDGAR Database	−19
Less firms in which entity is not an initial public offering	−3
Less firms in which executive turnover could not be determined due to firm delisting prior to restatement date	−4
Less firms in which executive turnover could not be determined due to lack of a CEO position in the firm	−5
Sample of Irregularity Firms	96
Sample of Irregularity Firms	96
Plus Control Firms (matched on industry and IPO date)	96
Sample of Irregularity Firms and Control Firms	192

The restatement announcements for the sample of 2,704 observations span the time period of January 1997 to June 2006. The founding dates of 8,464 firms with an initial public offering in the U.S. during 1975–2006 are obtained from Jay Ritter's website at: http://bear.cba.ufl.edu/ritter/ipodata.htm. Industry classifications are based on the 48 industry groupings in Fama and French (1997).

Next, we limit the restatement sample to observations for which we can identify the firm's IPO date from data provided on Jay Ritter's website.[18] This requirement reduces the sample to 309 observations. We then limit our sample to restatements that occurred within eight years of the company's IPO, which further reduces the sample to 165 newly listed publicly traded firms. The requirement that the restatement occurs within eight fiscal years of the firm's IPO accomplishes three objectives. First, this requirement increases the probability of finding a larger fraction of firms that are still managed by founder CEOs. Second, this requirement identifies firms in which the founder's manager-specific capital is valuable to the survival and success of the firm. Third, this requirement limits the sample size for data collection cost considerations.

Finally, we eliminate observations where Compustat or CRSP data are unavailable (resulting in a loss of 38 observations), the IPO prospectus cannot be found on EDGAR (eliminating 19 observations), the entity itself was not involved in the initial public offering (resulting in a loss of three observations), executive turnover cannot be determined due to firm delisting prior to the restatement date (reducing the sample by four observations), or executive turnover cannot be determined due to lack of a CEO position in the firm (eliminating five observations). These final sample selection requirements reduce our sample to

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[18] The founding dates of 8,464 firms with an initial public offering in the U.S. between 1975–2006 are obtained from Jay Ritter's website at: http://bear.cba.ufl.edu/ritter/ipodata.htm.

96 restatements related to accounting irregularities and occurring within eight years of the firm's IPO.

We then select a control sample by matching each of the 96 restatement firms to another IPO firm based on industry and closest IPO date, thereby identifying 96 control firms. We match on IPO date because our treatment sample contains relatively young firms, and our concern is that factors that influence the turnover decision may be correlated with firm age (e.g., board characteristics, growth rate, CEO ownership, etc.). Other factors likely to be related to turnover decisions (e.g., size) are controlled for in our regressions. We use the 48 industry groups classified in Fama and French (1997). Our total sample consists of 192 firms, split evenly between restatement firms and control firms. The indicator variable *IRREGULARITY* is assigned a value of 1 for the 96 restatement firms in our sample, and 0 for the 96 control firms.

To construct the variable *FOUNDER-CEO*, we review the biographies provided in the 10-K filings and/or the annual proxy statements. If the biography describes the CEO as one of the founders, then the CEO is classified as a founder. Our final sample of 192 firms contains 53 firms with founder CEOs. These 53 founder CEOs are split between 24 firms that reported accounting irregularities and 29 control firms. Our sample contains 139 firms with non-founder CEOs, of which 72 report accounting irregularities, while 67 comprise the control sample.

IV. DESCRIPTIVE STATISTICS AND RESULTS

Descriptive Statistics

Table 2, Panel A reports descriptive statistics for both the *IRREGULARITY* sample and the control sample, and Panel B reports similar descriptive statistics split by *FOUNDER-CEO* status. In both panels, *italics* indicate that the difference in means/medians between the irregularity and control firms is significant at the 5 percent level. As illustrated in Panel A, the distribution of founder CEOs is fairly consistent across the two groups of firms. About 25.0 percent of the firms in the irregularity sample have founder CEOs, while a slightly higher percentage of firms, 30.2 percent, in the control sample have founder CEOs. In the irregularity sample, the CEO turnover rate is 43.8 percent, while the CFO turnover rate is higher at 69.8 percent. In contrast, the executive turnover rates in the control sample are lower, with a CEO turnover rate of 10.4 percent and a CFO turnover rate of 18.8 percent. We also report the percentage of firms in which the CEO is retained but the CFO departs. This joint executive outcome is observed in 35.4 percent of the irregularity sample and 15.6 percent of the control sample.

Several performance indicators suggest that the firms in the irregularity sample tend to perform more poorly than firms in the control sample. For example, we measure each firm's buy-and-hold abnormal returns, starting from 250 days prior to the restatement announcement and ending eight days before the restatement announcement. The buy-and-hold returns, $BHAR_{[-250,-8]}$, are intended to capture the pre-restatement stock performance. Mean (median) $BHAR_{[-250,-8]}$ is −24.9 percent (−38.9 percent) for the irregularity firms and +3.6 percent (−1.8 percent) for the control sample. This finding is consistent with two scenarios: First, the irregularity sample experiences poor economic performance leading up to the restatement. Second, information about the irregularity firms' impending restatement is likely leaked and impounded into the stock price.

Mean (median) sales are 1,050.4 (133.1) for the irregularity sample and 251.9 (101.2) for the control sample. This sales distribution suggests that the irregularity firms are larger than the control firms, and sales are likely to be skewed in both samples. We also compute

TABLE 2
Descriptive Statistics

Panel A: Descriptive Statistics for Irregularity Firms versus Control Firms

	Irregularity Firms				Control Firms			
	n	Mean	Median	Std. Dev.	n	Mean	Median	Std. Dev.
FOUNDER-CEO	96	0.250	0.000	0.435	96	0.302	0.000	0.462
Percentage of Firms with:								
CEO Turnover	96	*0.438*	*0.000*	0.499	96	*0.104*	*0.000*	0.307
CFO Turnover	96	*0.698*	*1.000*	0.462	96	*0.188*	*0.000*	0.392
CEO Remaining and CFO Departing	96	*0.354*	*0.000*	0.481	96	*0.156*	*0.000*	0.365
CEO Age	96	49.5	49.0	8.4	96	49.4	49.0	7.9
CFO Age	85	44.6	44.0	7.1	95	45.9	45.0	6.9
Sales	96	1,050.4	133.1	3,977.9	96	251.9	101.2	424.3
Log(Sales + 1)	96	*5.041*	*4.899*	1.597	96	*4.528*	*4.627*	1.499
$BHAR_{[-250,-8]}$	96	*−0.249*	*−0.389*	0.565	96	*0.036*	*−0.018*	0.542
$BHAR_{[-1,+1]}$	96	*−0.261*	*−0.190*	0.336	96	*−0.029*	*−0.004*	0.186
$BHAR_{[+2,+250]}$	96	−0.158	*−0.484*	1.606	96	0.002	*−0.157*	0.810
ROA_{t-1}	96	*−0.182*	*−0.073*	0.276	96	*−0.059*	*0.012*	0.224
ROA_{t-1} − *Industry*	96	*0.274*	*0.185*	0.455	96	*0.408*	*0.340*	0.431
$CASHOP_{t-1}$	96	*−0.041*	*−0.005*	0.176	96	*0.037*	*0.035*	0.169
$CASHOP_{t-1}$ − *Industry*	96	*0.126*	*0.083*	0.242	96	*0.210*	*0.162*	0.247
CEO Equity%	96	10.8%	4.8%	13.5%	96	12.3%	5.5%	14.1%
Market/Book	96	*4.446*	2.216	4.654	96	*3.203*	2.134	3.298
R&D/Sales	96	0.192	0.042	0.360	96	0.234	0.000	0.451

Panel B: Descriptive Statistics for Irregularity Firms versus Control Firms, Split By Firms with Founder-CEOs

	Irregularity Firms				Control Firms			
	n	Mean	Median	Std. Dev.	n	Mean	Median	Std. Dev.
CEO Age								
FOUNDER-CEO	24	48.0	47.0	10.2	29	47.9	47.0	9.0
NON-FOUNDER-CEO	72	50.0	50.0	7.8	67	50.0	51.0	7.4
CFO Age								
FOUNDER-CEO	22	44.5	45.0	7.4	29	46.4	47.0	7.3
NON-FOUNDER-CEO	63	44.6	44.0	7.1	66	45.6	44.0	6.7
Sales								
FOUNDER-CEO	24	288.3	104.2	498.1	29	203.9	53.0	448.7
NON-FOUNDER-CEO	72	1,304.4	159.2	4,564.0	67	272.6	133.8	415.0
Log(Sales + 1)								
FOUNDER-CEO	24	4.563	4.653	1.574	29	4.155	3.989	1.475
NON-FOUNDER-CEO	72	*5.200*	5.072	1.583	67	*4.690*	4.904	1.491
$BHAR_{[-250,-8]}$								
FOUNDER-CEO	24	−0.325	*−0.520*	0.593	29	−0.041	*−0.035*	0.496
NON-FOUNDER-CEO	72	*−0.224*	*−0.318*	0.558	67	*0.069*	*0.001*	0.561

(*continued on next page*)

TABLE 2 (continued)

	Irregularity Firms				Control Firms			
	n	**Mean**	**Median**	**Std. Dev.**	**n**	**Mean**	**Median**	**Std. Dev.**
$BHAR_{[-1,+1]}$								
FOUNDER-CEO	24	*−0.259*	*−0.107*	0.393	29	*0.005*	*0.003*	0.065
NON-FOUNDER-CEO	72	*−0.262*	*−0.197*	0.318	67	*−0.044*	*−0.005*	0.217
$BHAR_{[+2,+250]}$								
FOUNDER CEO	24	0.469	−0.522	2.936	29	−0.044	−0.288	0.795
NON-FOUNDER-CEO	72	*−0.367*	*−0.484*	0.694	67	*0.022*	*−0.033*	0.822
ROA_{t-1}								
FOUNDER-CEO	24	−0.190	*−0.069*	0.291	29	−0.049	*0.020*	0.255
NON-FOUNDER-CEO	72	*−0.180*	*−0.075*	0.272	67	*−0.064*	*−0.010*	0.211
ROA_{t-1} − *Industry*								
FOUNDER-CEO	24	0.290	0.116	0.464	29	0.414	0.210	0.516
NON-FOUNDER-CEO	72	*0.268*	0.188	0.455	67	*0.406*	0.363	0.393
$CASHOP_{t-1}$								
FOUNDER-CEO	24	−0.091	−0.057	0.201	29	0.051	0.089	0.186
NON-FOUNDER-CEO	72	−0.024	−0.002	0.165	67	0.031	0.033	0.161
$CASHOP_{t-1}$ − *Industry*								
FOUNDER-CEO	24	0.089	0.008	0.283	29	0.234	0.210	0.300
NON-FOUNDER-CEO	72	0.138	0.086	0.227	67	0.199	0.160	0.222
CEO Equity%								
FOUNDER-CEO	24	20.2%	18.5%	14.628	29	21.1%	17.0%	15.855
NON-FOUNDER-CEO	72	7.7%	3.1%	11.656	67	8.5%	4.2%	11.433
Market/Book								
FOUNDER-CEO	24	5.005	2.950	4.939	29	3.475	1.892	3.812
NON-FOUNDER-CEO	70	4.254	2.040	4.573	66	3.084	2.181	3.070
R&D/Sales								
FOUNDER-CEO	24	0.219	0.165	0.343	29	0.265	0.026	0.501
NON-FOUNDER-CEO	72	0.183	0.011	0.368	67	0.221	0.000	0.431

Italics indicate that the difference in means/medians between the irregularity firms and the control firms is significant at the 5 percent level. The descriptive statistics are for the fiscal year prior to (1) the restatement date for the Irregularity Firms and (2) the identical date for each matched Control Firm.

Variable Definitions: Information about the firm's chief executive officer (CEO) and chief financial officer (CFO) is obtained from the 10-K, 10-Q, 8-K, and Proxy Statements. Descriptions of the firm's current CEO and CFO are obtained as of the six months prior to (1) the restatement date for the Irregularity Firms and (2) the identical date for each matched Control Firm. *FOUNDER-CEO* = 1 if the firm's CEO is described as a founder, 0 otherwise. Turnover information for the firm's current CEO and CFO are obtained as of the six months after (1) the restatement date for the Irregularity Firms and (2) the identical date for each matched Control Firm. The following data are from Compustat unless otherwise specified. *Sales* = data#12. *Log*(*Sales* + 1) = log(data#12 + 1). $BHAR_{[-250,-8]}$ = Buy-and-Hold Return for the firm − Buy-and-Hold Return for the S&P 500 Index = [(price$_{t-8}$/price$_{t-250}$) − 1] − [(S&P500 Index$_{t-8}$/S&P500 Index$_{t-250}$) − 1], as calculated for the period prior to (1) the restatement date for the Irregularity Firms and (2) the identical date for each matched Control Firm. Abnormal buy-and-hold returns are also calculated for (1) the three-day window around the restatement announcement, as captured by $BHAR_{[-1,+1]}$, and (2) the post-restatement year, as captured by $BHAR_{[+2,+250]}$; delisting returns are set to −100 percent. ROA_{t-1} = return on assets in the year prior to restatement = data#18/lagged total assets. $CASHOP_{t-1}$ = cash from operations in the year prior to restatement = data#308/lagged total assets. *Industry* = annual mean ROA_{t-1} or $CASHOP_{t-1}$ for each of the 48 industry groupings in Fama and French (1997). *CEO Equity%* = percentage of common stock owned by the CEO as of six months prior to (1) the restatement date for the Irregularity Firms and (2) the identical date for each matched Control Firm. *Market/Book* = market value of equity/book value of equity = (data#24 * data#25)/data#60. *R&D/Sales* = data#46/data#12. The quantitative variables *Log*(*Sales* + 1), $BHAR_{[-250,-8]}$, ROA_{t-1}, and $CASHOP_{t-1}$ are used as controls and winsorized at +/− 5 percent.

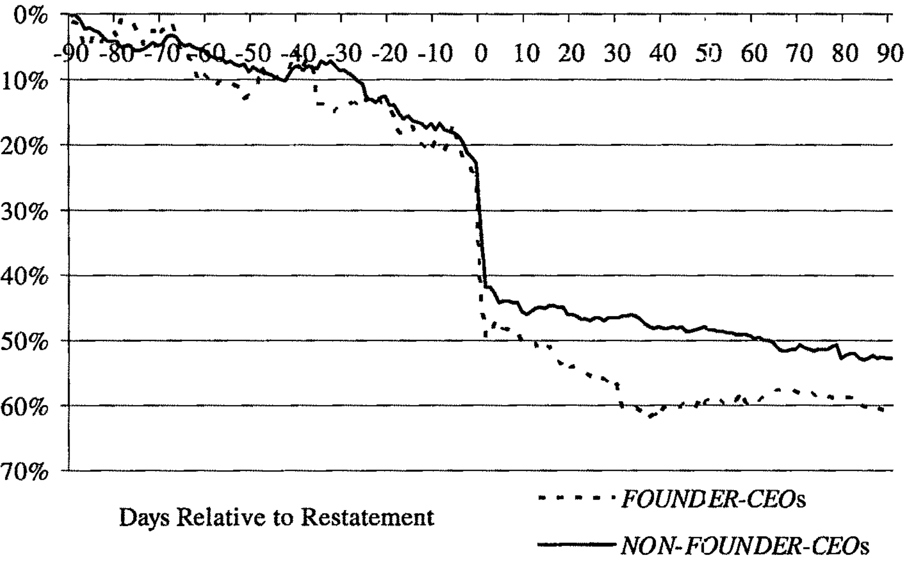
ROA_{t-1} as net income (Compustat #18) in the year prior to the restatement divided by total assets (Compustat #6) at the start of that year. Mean (median) ROA_{t-1} is −18.2 percent (−7.3 percent) for the irregularity sample and −5.9 percent (+1.2 percent) for the control sample. Similarly, cash from operations ($CASHOP_{t-1}$) is also lower for irregularity firms than for the control sample. Mean (median) cash from operations is −4.1 percent (−0.5 percent) for the irregularity firms and 3.7 percent (3.5 percent) for the control sample.

Panel B of Table 2 shows descriptive statistics for the irregularity and control samples, both split by founder CEO status. Overall, the descriptive statistics reported in Panel B for the founder versus non-founder sample are similar to those reported in Panel A, with the exception of $BHAR_{[-250,-8]}$, and $CASHOP_{t-1}$. Both $BHAR_{[-250,-8]}$, and $CASHOP_{t-1}$ suggest that the founder CEO firms experience worse performance.

Stock Returns

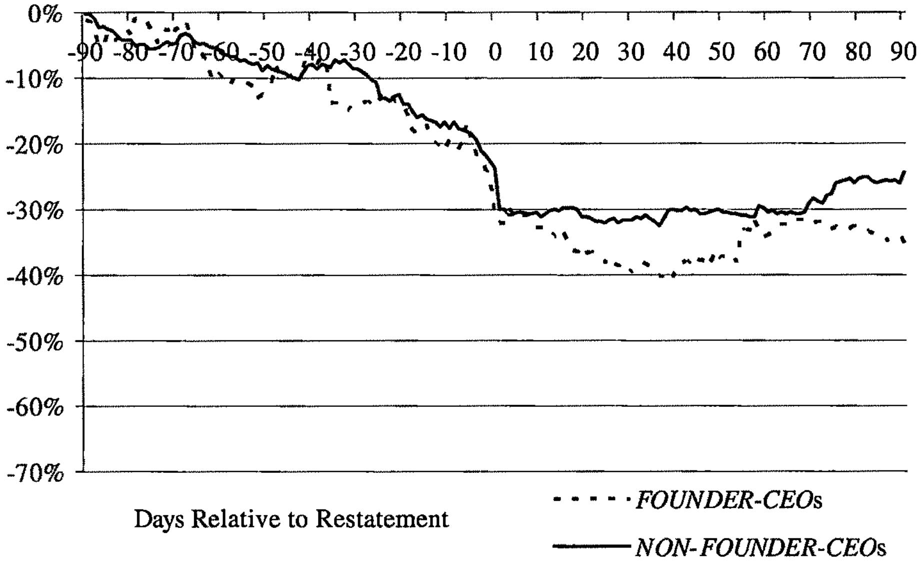
Figures 1 and 2 show plots of buy-and-hold abnormal returns (*BHAR*) for the irregularity sample only, from 90 trading days prior to 90 trading days after the restatement announcement. We plot *BHAR*s separately for firms managed by founder CEOs and firms managed by non-founder CEOs. Figure 1 plots *BHAR*s with delisting returns set to −100 percent after the restatement announcement, and Figure 2 reports a similar plot but with

FIGURE 1
Abnormal Returns for 180 Days around Restatement Announcements
(Delisted Firm Returns Are Set to −100%)



Abnormal Returns are calculated for days [−90, +90] around the restatement date for the Irregularity Firms. *BHAR* = Buy-and-Hold Return for the firm − Buy-and-Hold Return for the S&P 500 Index. The 96 firms represented in this figure consist of 24 firms with *FOUNDER-CEOs* and 72 firms with *NON-FOUNDER-CEOs*.

FIGURE 2
Abnormal Returns for 180 Days around Restatement Announcements
(Delisted Firm Returns Are Set to 0%)



Abnormal Returns are calculated for days [−90, +90] around the restatement date for the Irregularity Firms. *BHAR* = Buy-and-Hold Return for the firm − Buy-and-Hold Return for the S&P 500 Index. The 96 firms represented in this figure consist of 24 firms with *FOUNDER-CEOs* and 72 firms with *NON-FOUNDER-CEOs*.

delisting returns set to 0 percent.[19] These two extreme solutions to handling the returns of delisted firms provide upper and lower bounds for any potential measurement problems associated with these returns.

Consistent with the findings of prior research, restatement announcements prompt a large negative market reaction (Hribar and Jenkins 2006; Palmrose et al. 2004). Additionally, returns appear to drift downward prior to the restatement announcement. This downward drift in returns is likely attributed to problems in identifying the exact date of the restatement announcement. The GAO database records the date that the company announces its intent to restate its financials. However, some firms may choose to make pre-announcements about initiating an investigation into a potential accounting problem. Thus, for these firms, information about the firm's accounting irregularities have already been released prior to the official "intent to restate" announcement date listed in the GAO database. As a result, measuring the firm's returns around the restatement date as listed in the GAO database potentially understates the full stock price impact of restatement-related news.

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[19] We begin our returns analysis with a total of 96 restatement firms, of which 24 had founder CEOs, while 72 firms had non-founder CEOs. By day +90 after the restatement announcement, we lose six founder CEO firms and 16 non-founder CEO firms. Many of these firms temporarily stopped trading because delayed filings caused these firms to be out of compliance with the exchange listing rules.

The returns patterns for founder CEO firms and non-founder CEO firms are similar, providing reasonable assurance that the severity of the accounting irregularities is not likely to be different for founder versus non-founder firms.

Contingency Tests

Table 3 reports executive turnover contingency tables, split according to whether the firm has an *IRREGULARITY* and whether the firm has a *FOUNDER-CEO*. Panel A reports the CEO turnover rates. Turnover rates for the control sample are similar for founder CEO firms versus non-founder CEO firms, and these rates also appear to be consistent with those reported in previous studies. For the irregularity sample, the turnover rate for founder CEO firms is only 29.17 percent, as compared to 48.61 percent for non-founder CEO firms. This

TABLE 3
Percentage of Firms with CEO and CFO Turnover around Restatements

Panel A: Percentage of Firms with CEO Turnover

	Irregularity Firms	Control Firms	Difference
FOUNDER-CEO	29.17% n = 24	10.34% n = 29	18.82%###
NON-FOUNDER-CEO	48.61% n = 72	10.45% n = 67	38.16%###
Difference	−19.44%#	0.10%	

Panel B: Percentage of Firms with CFO Turnover

	Irregularity Firms	Control Firms	Difference
FOUNDER-CEO	83.33% n = 24	13.79% n = 29	69.54%###
NON-FOUNDER-CEO	65.28% n = 72	20.90% n = 67	44.38%###
Difference	18.06%#	−7.10%	

Panel C: Percentage of Firms with CEO Remaining and CFO Departing

	Irregularity Firms	Control Firms	Difference
FOUNDER-CEO	58.33% n = 24	10.34% n = 29	47.99%###
NON-FOUNDER-CEO	27.78% n = 72	17.91% n = 67	9.87%#
Difference	30.56%###	−7.57%	

#, ##, ### Significantly different at p-value $< .10$, $< .05$, and $< .01$, respectively, based on a two-tailed binomial test of proportions.

Variable Definitions: Information about the firm's chief executive officer (CEO) and chief financial officer (CFO) is obtained from the 10-K, 10-Q, 8-K, and Proxy Statements. Descriptions of the firm's current CEO and CFO are obtained as of the six months prior to (1) the restatement date for the Irregularity Firms and (2) the identical date for each matched Control Firm. *FOUNDER-CEO* = 1 if the firm's CEO is described as a founder, 0 otherwise. Turnover information for the firm's current CEO and CFO are obtained as of the six months after (1) the restatement date for the Irregularity Firms and (2) the identical date for each matched Control Firm.

difference in turnover rates of 19.44 percent is statistically significant at a two-tailed p-value of 0.066. Consistent with H1, these univariate results suggest that founder CEOs are less likely than non-founder CEOs to be terminated when an accounting irregularity is disclosed. Within firms with founder CEOs, the difference in turnover rates between irregularity firms and control firms is 18.83 percent (29.17 percent − 10.34 percent), and this difference is statistically significant at the 1 percent level. In contrast, within non-founder CEO firms, the difference in CEO turnover for the irregularity sample compared to the control sample is 38.16 percent (48.61 percent − 10.45 percent), which is again significant at the 1 percent level.

Table 3, Panel B reports CFO turnover rates. For the control sample, the turnover rate for CFOs employed within founder CEO firms is 13.79 percent, which is lower than the 20.90 percent turnover rate for CFOs employed in non-founder CEO firms. This difference is not statistically significant. The relatively high turnover rate of 20.90 percent for CFOs in non-founders CEO firms likely reflects post-IPO voluntary turnover.[20] Within the irregularity sample, the CFO turnover rate for founder CEO firms is 83.33 percent (20 out of 24 cases), as compared to 65.28 percent (47 out of 72 cases) for non-founder CEO firms. The direction of this difference of 18.05 percent (83.33 percent − 65.28 percent) is consistent with our predictions, and is marginally significant (two-tailed $p = 0.0843$).

In Table 3, Panel C, we report the proportion of firms in which the CEO is retained but the CFO departs. As predicted in H3, for founder CEO firms, the proportion of cases with this outcome involves 58.33 percent of the irregularity sample and 10.34 percent of the control sample. This difference of 47.99 percent (58.33 − 10.34 percent) is statistically significant (two-tailed $p < 0.01$). Additionally, within the irregularity sample only, this outcome occurs in 58.33 percent of founder CEO firms and 27.78 percent of non-founder CEO firms. We next report results of multivariate tests that control for other factors likely to be related to executive turnover.

Multivariate Tests

Table 4 reports the results from logistic regressions involving our executive turnover models, which we detail in models (1), (2), and (3). Tests of H1 involve CEO turnover, which occurs in 42 irregularity firms and 10 control firms. Column 1 reports CEO turnover results with ROA_{t-1} as a financial performance measure. Consistent with our predictions, we find that the coefficient on *IRREGULARITY* (β_1) is positive (+2.397) and significantly different from zero (one-tailed $p < .01$). As in our univariate tests, the coefficient (β_2) on *FOUNDER-CEO* is not significant. This suggests that for the control sample in which no irregularities are reported, the turnover rate for founder CEO firms is no different from the rate in non-founder CEO firms. Consistent with H1, the coefficient β_3 on the interaction term *IRREGULARITY* * *FOUNDER-CEO* is negative (−1.131) and significant (one-tailed $p = .049$). That is, the effect of an irregularity on turnover is significantly dampened when the CEO is also the founder of the firm.

One problem with using ROA as a control for performance is that because the numerator of ROA is earnings, which most likely contains the effect of any misstatement, this

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[20] We considered ways to distinguish between voluntary and involuntary executive turnover (e.g., Parrino 1997), but this is somewhat challenging for CFOs. First, it appears anecdotally that CFOs seek new employment with new IPO firms, thereby leaving firms for alternate employment opportunities as opposed to retiring. Additionally, information about CFO departures is fairly limited compared to CEO departures, particularly in earlier years after the IPO. Moreover, requiring explicit CFO departure dates to determine the nature of CFO turnover decisions would likely reduce the sample.

TABLE 4
The Relation between Founder CEO Influence and Executive Turnover

Logit Model with Executive Turnover Outcome as the Dependent Variable with Standard Errors Clustered by IPO Year

		CEO Turnover			CFO Turnover			CEO Remains and CFO Departs	
	Pred.	(1)	(2)	**Pred.**	(3)	(4)	**Pred.**	(5)	(6)
Intercept		−0.731	−0.848		−1.392	−1.974*		−2.795***	−3.164***
		(1.20)	(1.43)		(1.63)	(1.94)		(3.53)	(3.61)
IRREGULARITY	+	2.397***	2.376***	+	2.281***	2.284***	+	0.173	0.086
		(4.12)	(3.90)		(4.26)	(3.48)		(0.47)	(0.20)
FOUNDER-CEO	−	−0.264	−0.216		−0.504	−0.442		−0.484	−0.453
		(0.47)	(0.40)		(0.72)	(0.73)		(0.70)	(0.71)
*IRREGULARITY * FOUNDER-CEO*	−	−1.131**	−1.265**	+	1.431**	1.225*	+	2.149***	2.001**
		(1.66)	(1.93)		(1.99)	(1.83)		(2.40)	(2.30)
ANNUAL	+	−0.518	−0.546		−0.505	−0.643		0.428	0.363
		(1.24)	(1.43)		(0.81)	(0.91)		(0.94)	(0.69)
SUBSIDIARY	−	1.312	1.402*		−0.047	0.306		−1.110	−0.903
		(1.63)	(1.68)		(0.07)	(0.40)		(1.48)	(1.24)
Log(*Sales* + 1)		−0.326***	−0.290**		−0.009	0.144		0.244**	0.351**
		(3.16)	(2.53)		(0.07)	(0.88)		(2.18)	(2.46)

(*continued on next page*)

TABLE 4 (continued)

Logit Model with Executive Turnover Outcome as the Dependent Variable with Standard Errors Clustered by IPO Year

		CEO Turnover			CFO Turnover			CEO Remains and CFO Departs	
	Pred.	(1)	(2)	**Pred.**	(3)	(4)	**Pred.**	(5)	(6)
$BHAR_{[-250,-8]}$	–	−0.656**	−0.668**		0.023	−0.049		0.266	0.219
		(1.93)	(2.24)		(0.07)	(0.16)		(0.89)	(0.75)
ROA_{t-1}	–	−0.388			−1.257***			−1.103*	
		(0.53)			(3.20)			(1.91)	
$CASHOP_{t-1}$	–		−1.198			−4.577***			−3.080***
			(1.48)			(3.26)			(2.58)
n		192	192		192	192		192	192
Pseudo R^2		21.74%	22.04%		23.27%	27.17%		11.38%	13.12%

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*, **, *** Significant at 10 percent, 5 percent, and 1 percent, respectively. A one-tailed significance test is used in cases where a prediction is made on the coefficient; otherwise a two-tailed significance test is used.

Robust t-statistics (in parentheses) use a Huber-White correction for general heteroscedasticity in the standard errors.

Variable Definitions: *IRREGULARITY* = 1 if the firm had a restatement that was classified as an accounting irregularity within eight fiscal years of IPO, 0 otherwise. Descriptions of the firm's current CEO and CFO are obtained as of the six months prior to (1) the restatement date for the Irregularity Firms and (2) the identical date for each matched Control Firm. *FOUNDER-CEO* = 1 if the firm's CEO is described as a founder, 0 otherwise. Turnover information for the firm's current CEO and CFO are obtained as of the six months after (1) the restatement date for the Irregularity Firms and (2) the identical date for each matched Control Firm. *ANNUAL* = 1 if the firm's restatement involved the annual financial statements (10-K filing), 0 if the firm's restatement involved the quarterly financial statements (10-Q filing). *SUBSIDIARY* = 1 if the firm's restatement occurred at the subsidiary level, 0 otherwise. $Log(Sales + 1)$ = log(data#12 + 1). $BHAR_{[-250,-8]}$ = Buy-and-Hold Return for the firm − Buy-and-Hold Return for the S&P 500 Index = $[(price_{t-8}/price_{t-250}) - 1] - [(S\&P500\ Index_{t-8}/S\&P500\ Index_{t-250}) - 1]$, as calculated for the period prior to (1) the restatement date for the Irregularity Firms and (2) the identical date for each matched Control Firm. ROA_{t-1} = return on assets in the year prior to restatement = data#18/lagged total assets. $CASHOP_{t-1}$ = cash from operations in the year prior to restatement = data#308/lagged total assets. The quantitative variables $Log(Sales + 1)$, $BHAR_{[-250,-8]}$, ROA_{t-1}, and $CASHOP_{t-1}$ are used as controls and winsorized at +/− 5 percent.

particular variable contains measurement error for the irregularity firms. Therefore, in Column 2 of Table 4, we report logistic results after replacing ROA_{t-1} with cash flow ($CASHOP_{t-1}$) as an alternate measure of the firm's financial performance. Our results when cash from operations is used are virtually identical to the results that obtain when ROA is used in the regression.

In Table 4, Columns 3 and 4, we report the results for model (2), using CFO turnover as the dependent variable. CFO turnover occurs in 67 irregularity firms and 18 control firms. Similar to the previous results involving irregularities and CEO turnover, the coefficient (β_1) on *IRREGULARITY* is positive (+2.281) and significant (one-tailed $p < .01$). This finding suggests that the probability of CFO turnover increases when an accounting irregularity is reported. As predicted in H2, the coefficient on *IRREGULARITY * FOUNDER-CEO* (β_3) is positive (+1.431) and significant (one-tailed $p = .027$). This suggests that the probability of CFO turnover in the presence of an accounting irregularity increases when the CEO is a founder.[21] As shown in Column 4 of Table 4, results are similar when $CASHOP_{it}$ is substituted for ROA_{it}, though the robust t-statistic is slightly smaller. One interpretation of this empirical result is that the boards of firms with founder CEOs are more likely to place the blame for the irregularity on the CFO, as opposed to the CEO.

Table 4, Columns 5 and 6, reflect estimates of model (3), in which we test whether founder firms use the CFO as a scapegoat to protect the CEO. In this model, the dependent variable reflects the joint executive outcome in which the CEO is retained but the CFO departs; this outcome occurs in 34 irregularity firms and 15 control firms. The variable *CFO_NOCEO_TURNOVER* is set to 1 if the CEO is retained but the CFO departs, and 0 for all other cases. Consistent with H3, the coefficient on *IRREGULARITY * FOUNDER-CEO* (β_3) is consistent with boards shifting the blame *away from* the CEO and *toward* the CFO in founder-managed firms with accounting irregularities. Put simply, such blame shifting is no more likely to occur after an accounting irregularity, unless the CEO is the founder.

We also compute the predicted probability of executive turnover, as documented in Table 5. We first examine the predicted probability of CEO turnover for three specific cases (Table 5, Panel A). We first examine cases in which the firm has no irregularities and a non-founder CEO (Case #1), and in this case the CEO turnover probability is 9.33 percent. We then examine cases in which the firm has an accounting irregularity and a non-founder CEO (Case #2), and in these instances, the CEO turnover probability increases to 52.54 percent. Finally, we examine cases in which the firm has an accounting irregularity and a founder CEO (Case #3). In this last case, which is shaded in Panel A of Table 5, the predicted turnover probability decreases to 20.11 percent. This drastic decrease in CEO turnover rates for Case #3 compared to Case #2 is consistent with our hypothesis that CEO founders hold a unique position within the firm.

Additional Tests

In this section, we conduct additional tests to rule out potential alternative explanations for our results. In Table 6, we present these regression results for our turnover models,

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[21] Consistent with Fee and Hadlock (2004), we find that CFO turnover is, on average, no different for CFOs who work for founder-managed firms. However, when an accounting irregularity occurs, CFOs are actually more likely to be fired when the CEO is a founder.

TABLE 5
Predicted Probability of Executive Turnover

Panel A: Predicted Probability of CEO Turnover For the Following Cases

	Case #1	Case #2	Case #3
IRREGULARITY =	0	1	1
FOUNDER-CEO =	0	0	1
IRREGULARITY * *FOUNDER-CEO* =	0	0	1
Turnover Probability	9.33%	52.54%	20.11%

Panel B: Predicted Probability of CFO Turnover For the Following Cases

	Case #1	Case #2	Case #3
IRREGULARITY =	0	1	1
FOUNDER-CEO =	0	0	1
IRREGULARITY * *FOUNDER-CEO* =	0	0	1
Turnover Probability	18.33%	68.77%	82.82%

Panel C: Predicted Probability of CEO Remaining and CFO Departing For the Following Cases

	Case #1	Case #2	Case #3
IRREGULARITY =	0	1	1
FOUNDER-CEO =	0	0	1
IRREGULARITY * *FOUNDER-CEO* =	0	0	1
Turnover Probability	19.31%	20.68%	55.10%

Variable Definitions: *IRREGULARITY* = 1 if the firm had a restatement that was classified as an accounting irregularity within eight fiscal years of IPO, 0 otherwise. *FOUNDER-CEO* = 1 if the firm's CEO is described as a founder, 0 otherwise.

including $CASHOP_{t-1}$ as the measure of the firm's financial performance.[22] First, we consider whether our founder results are driven by entrenchment caused by high CEO ownership, as opposed to entrenchment that is specific to founders. To disentangle the ownership effect from the founder effect, we include *CEO Equity%* and *IRREGULARITY* * *CEO Equity%* in the executive turnover models, where *CEO Equity%* is defined as the percentage of shares owned by the CEO. We predict that higher CEO ownership will *decrease* the likelihood of CEO turnover.

As shown in Table 6, we find that in the full sample of firms shown in Column 1, the coefficient on *IRREGULARITY* * *CEO Equity%* is not significantly different from zero in the CEO turnover regression. Meanwhile, the coefficient on *IRREGULARITY* * *FOUNDER-CEO*, our primary variable of interest, remains relatively unchanged compared to the results reported in Table 4. Similarly, we also perform the analysis on the sample of irregularity firms only. The coefficient on *CEO Equity%* shown in Column 2 is not significantly different from zero, but in the same regression, the coefficient on *FOUNDER-CEO* of −1.281 is negative and significant (one-tailed $p < .01$). Similar results obtain when we examine these

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[22] Recall that one problem with using ROA as a control for performance is that because the numerator of ROA is earnings, which most likely contains the effect of any misstatement, this particular variable contains measurement error for the irregularity firms. Therefore, we use cash flow ($CASHOP_{t-1}$) as an alternate measure of the firm's financial performance.

TABLE 6

The Relation between Founder CEO Influence and Executive Turnover, After Controlling for CEO Equity Ownership, CEO Accounting Expertise, and Board Independence

		Logit Model with Executive Turnover Outcome as the Dependent Variable With Standard Errors Clustered by IPO Year						Logit Model with Executive Turnover Outcome as the Dependent Variable with Standard Errors Clustered by IPO Year	
		CEO Turnover			CFO Turnover			CEO Remains and CFO Departs	
		(1)	(2)		(3)	(4)		(5)	(6)
	Pred.	Full Sample	Irregularity Sample Only	Pred.	Full Sample	Irregularity Sample Only	Pred.	Full Sample	Irregularity Sample Only
Intercept		−3.933***	3.290**		−1.759	2.416*		−3.402	−3.241**
		(3.15)	(1.98)		(0.63)	(1.91)		(1.33)	(2.14)
IRREGULARITY	+	7.043***		+	3.155		+	−0.012	
		(4.86)			(1.11)			0.00	
FOUNDER-CEO	−	−0.002	−1.281***	+	−0.624	0.971*	+	−0.734	1.768***
		0.00	(2.94)		(1.04)	(1.63)		(0.98)	(5.10)
*IRREGULARITY * FOUNDER-CEO*	−	−1.302**		+	1.692**		+	2.438***	
		(2.19)			(2.07)			(2.77)	
ANNUAL	+	−0.587	−0.572*		−0.635	−0.649		0.331	0.328
		(1.58)	(1.71)		(0.95)	(0.94)		(0.66)	(0.63)
SUBSIDIARY	−	1.318	1.236		0.250	0.247		−0.968	−1.087
		(1.58)	(1.59)		(0.33)	(0.35)		(1.16)	(1.32)
Log(*Sales*+1)		−0.275**	−0.269		0.133	−0.002		0.350**	0.365**
		(2.39)	(1.35)		(0.76)	(0.01)		(2.37)	(2.26)
$BHAR_{[-250,-8]}$	−	−0.770***	−0.301		0.102	0.825		0.293	0.545
		(2.95)	(0.95)		(0.29)	(1.45)		(0.95)	(1.57)
$CASHOP_{t-1}$	−	−1.138	−1.663		−4.739***	−5.447***		−3.147***	−2.363***
		(1.17)	(1.62)		(3.52)	(2.71)		(2.74)	(2.10)

(*continued on next page*)

TABLE 6 (continued)

	Logit Model with Executive Turnover Outcome as the Dependent Variable With Standard Errors Clustered by IPO Year						Logit Model with Executive Turnover Outcome as the Dependent Variable with Standard Errors Clustered by IPO Year		
		CEO Turnover			CFO Turnover			CEO Remains and CFO Departs	
		(1)	(2)		(3)	(4)		(5)	(6)
	Pred.	**Full Sample**	**Irregularity Sample Only**	**Pred.**	**Full Sample**	**Irregularity Sample Only**	**Pred.**	**Full Sample**	**Irregularity Sample Only**
CEO Equity%	–	−0.003	−0.029	+	0.017	−0.016	+	0.021	−0.003
		(0.30)	(1.50)		(0.87)	(0.97)		(1.04)	(0.26)
*IRREGULARITY * CEO Equity%*	–	−0.023		+	−0.034		+	−0.023	
		(1.17)			(1.40)			(1.49)	
Accounting Expertise	+	0.017	0.316		0.630*	−1.383**		0.492*	−0.681
		(0.02)	(0.45)		(1.80)	(2.19)		(1.75)	(1.12)
*IRREGULARITY * Accounting Expertise*	+	0.443			−1.712***			−1.045	
		(0.36)			(2.68)			(1.58)	
Board Outsider%	+	4.228***	−2.323		−0.671	−0.935	–	−0.065	0.507
		(2.89)	(1.47)		(0.23)	(0.66)		(0.02	(0.38)
*IRREGULARITY * Board Outsider%*	+	−6.447***			−0.201		–	0.772	
		(2.93)			(0.06)			(0.21)	
n		192	96		192	96		192	96
Pseudo R^2		25.15%	15.02%		29.62%	17.06%		14.34%	13.68%

(continued on next page)

TABLE 6 (continued)

*, **, *** Significant at 10 percent, 5 percent, and 1 percent, respectively. A one-tailed significance test is used in cases where a prediction is made on the coefficient; otherwise a two-tailed significance test is used.

Robust t-statistics (in parentheses) use a Huber-White correction for general heteroscedasticity in the standard errors.

Variable Definitions: *IRREGULARITY* = 1 if the firm had a restatement that was classified as an accounting irregularity within eight fiscal years of IPO, 0 otherwise. Descriptions of the firm's current CEO and CFO are obtained as of the six months prior to (1) the restatement date for the Irregularity Firms and (2) the identical date for each matched Control Firm. *FOUNDER-CEO* = 1 if the firm's CEO is described as a founder, 0 otherwise. Turnover information for the firm's current CEO and CFO are obtained as of the six months after (1) the restatement date for the Irregularity Firms and (2) the identical date for each matched Control Firm. *ANNUAL* = 1 if the firm's restatement involved the annual financial statements (10-K filing), 0 if the firm's restatement involved the quarterly financial statements (10-Q filing). *SUBSIDIARY* = 1 if the firm's restatement occurred at the subsidiary level, 0 otherwise. *Log(Sales* + 1) = log(data#12 + 1). $BHAR_{[-250,-8]}$ = Buy-and-Hold Return for the firm − Buy-and-Hold-Return for the S&P 500 Index = [($price_{t-8}/price_{t-250}$) − 1] − [(S&P500 $Index_{t-8}$/S&P500 $Index_{t-250}$) − 1], as calculated for the period prior to (1) the restatement date for the Irregularity Firms and (2) the identical date for each matched Control Firm. ROA_{t-1} = return on assets in the year prior to restatement = data#18/lagged total assets. $CASHOP_{t-1}$ = cash from operations in the year prior to restatement = data#308/lagged total assets. *CEO Equity%* = percentage of common stock owned by the CEO as of six months prior to (1) the restatement date for the Irregularity Firms and (2) the identical date for each matched Control Firm. *Accounting Expertise* = 1 if the CEO's biography states that he/she (1) holds an accounting degree, (2) has previous experience as a chief financial officer, or (3) has had previous employment or educational experience in a financial-related area. *Board Outsider%* = percentage of outside members of the board of directors as of six months prior to the restatement date; board members are classified as outsiders if they (1) are not currently employed by the firm, (2) have not held previous positions with the firm, (3) are not related to the CEO or to the founder, and (4) are not founders of the firm. The quantitative variables *Log(Sales* + 1), $BHAR_{[-250,-8]}$, ROA_{t-1}, $CASHOP_{t-1}$, and *CEO Equity%* are used as controls and winsorized at +/− 5 percent.

variables in the tests shown in Columns 5 and 6 of Table 6. This evidence suggests that the *FOUNDER-CEO* effect is distinct in nature from the *CEO Equity%* effect.

The second alternative explanation for our results is that founder CEOs have less accounting expertise, and are less involved in accounting decisions, than non-founder CEOs. If founders CEOs are not involved with accounting decisions, then they are less likely to be aware of, or involved with, the accounting irregularity. Therefore, the lower turnover rates for founder CEOs would simply reflect the fact that founder CEOs are less likely to be responsible for the irregularity. To explore this alternative explanation for our findings, we create a variable to broadly capture the CEO's level of accounting and/or finance knowledge. We define the indicator variable *Accounting Expertise* as equal to 1 if the CEO's biography states that s/he (1) holds an accounting degree, (2) has previous experience as a chief financial officer, or (3) has had previous employment or educational experience in a financial-related area.[23] We include Accounting Expertise and *IRREGULARITY * Accounting Expertise* in our executive turnover models. This alternative explanation predicts that the CEO's *Accounting Expertise increases* the probability of turnover during an accounting irregularity.

Since the *Accounting Expertise* of the CEO directly affects the turnover rates of the CEO—and is not likely to directly affect the turnover rates for CFOs—we examine and discuss CEO turnover rates only. Recall that the CEO turnover regression in Table 6, Column 1 involves the full sample of firms, whereas the CEO turnover regression in Table 6, Column 2 is applied to the irregularity sample only. In Column 1, the coefficient on *IRREGULARITY * Accounting Expertise* is not significantly different from zero. In Column 2, the coefficient on *Accounting Expertise* is also not significantly different from zero. Furthermore, the results for the CFO scapegoat scenario reveal that the coefficients on *IRREGULARITY * Accounting Expertise* (in Column 5) and *Accounting Expertise* (in Column 6) are negative, which is opposite the sign that one would predict under the alternate hypothesis. These results demonstrate that the board's decisions to retain the founder CEO are not due to the lack of accounting expertise.

A third alternative explanation for our results is that founder CEOs are likely to populate their firm's board of directors with a smaller percentage of outside directors, thereby creating a board with weak governance mechanisms. Under this alternate explanation, firms with weak (strong) boards are less likely (more likely) to fire a founder CEO. To explore this possibility, we create a variable *Board Outsider%* that measures the percentage of outside members of the board of directors as of six months prior to the restatement date; board members are classified as outsiders if they (1) are not currently employed by the firm, (2) have not held previous positions with the firm, (3) are not related to the CEO or to the founder, and (4) are not founders of the firm. We consider *Board Outsider%* to proxy for board independence. Firms with stronger governance, as captured by higher *Board*

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[23] We note that our definition of *Accounting Expertise* differs from the extensive definition of a "Financial Expert" as outlined in Section 407 of the Sarbanes-Oxley Act of 2002. Section 407 states that to qualify as a "Financial Expert," several factors are considered, including: (1) whether a person has, through education and experience as a public accountant or auditor or a principal financial officer, comptroller, or principal accounting officer of an issuer, or from a position involving the performance of similar functions; (2) an understanding of generally accepted accounting principles and financial statements; (3) experience in the preparation or auditing of financial statements of generally comparable issuers; (4) experience in the application of such principles in connection with the accounting for estimates, accruals, and reserves; (5) experience with internal accounting controls; and (6) an understanding of audit committee functions. Since a significant portion of our sample pre-dates Sarbanes-Oxley, we are unable to implement the SOX criteria; thus, we established the criteria indicated and reviewed the information disclosed in CEO biographies to ascertain the CEO's *Accounting Expertise.*

Outsider%, are *more* likely to fire a CEO. We include *Board Outsider%* and *IRREGULARITY * Board Outsider%* in our turnover models, and under this alternate explanation, the signs on these coefficients are expected to be positive.

Table 6 shows the empirical results involving the effect of *Board Outsider%* on executive turnover. In Column 1, the coefficient on *IRREGULARITY * Board Outsider%* is negative, which is opposite the positive sign that one would expect under the alternate explanation. Similarly, in Column 2, the coefficient on *Board Outsider%* of -2.323 is also negative. Note that in these regressions, our main results involving *IRREGULARITY * FOUNDER-CEO* in Column 1 and *FOUNDER-CEO* in Column 2 continue to hold, as predicted. In the CFO scapegoat test results, shown in Columns 5 and 6, the coefficients on *IRREGULARITY * Board Outsider%* and *Board Outsider%* are not significantly different from zero. These results demonstrate that the low turnover rates of founder CEOs are not driven by the composition of the board of directors.

Fourth, we consider whether our results are caused by possible sampling error in our control sample. To address this concern, we use only the 96 observations from the irregularity sample to test our main hypotheses and the alternate explanations involving *CEO Equity%*, *Accounting Expertise*, and *Board Outsider%*. Confining our predictions to the 96 firms in the irregularity sample allows us to drop the interaction terms involving *IRREGULARITY* and to focus on the predictions involving *FOUNDER-CEO*. As previously discussed, the empirical results shown in Table 6, Columns 2, 4, and 6, which involve the variables *CEO Equity%*, *Accounting Expertise*, and *Board Outsider%* show that the empirical relations between *FOUNDER-CEO* status and executive turnover remain robust and unchanged. In some cases, the relations involving *FOUNDER-CEO* become much stronger in statistical significance.

Finally, we examine whether the fired-CFOs of founder managed firms are likely to be designated as "scapegoats." "Scapegoat" CFOs would likely have better subsequent employment opportunities than CFOs that were fired for poor performance. Our untabulated tests reveal that the fired-CFOs of founder managed firms are (1) less likely to be the subject of an AAER and (2) more likely to find subsequent employment in a private or public firm, compared to fired-CFOs of non-founder firms.[24] These findings support the CFO "scapegoat" possibility in founder CEO firms.

Collectively, our findings are consistent with the board of directors recognizing that replacing the founder CEO with the next best manager will reduce firm value. At the same time, the board is compelled to explore different methods by which to restore financial reporting credibility. One of these methods may involve placing blame for the alleged wrongdoing on the CFO, as opposed to the CEO.

V. CONCLUSION

In this study, we examine whether the board's response to accounting irregularities differs when the CEO is a founder. Specifically, we examine whether founder CEOs are able to avoid being terminated in cases where the firm discloses an accounting irregularity.

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[24] To identify the subsequent employment of departing CFOs, we search multiple sources. First, we read through the firm's 10-K, 10-Q, 8-K, and Proxy Statements filings from the SEC's EDGAR database for the one-year and two-year window subsequent to the irregularity disclosure. Second, we conduct a search through the *Wall Street Journal* website, http://online.wsj.com/home-page, for the executive's last name, last name with first initial, and last name and former-firm name. Third, we conduct a broader, more general search through Yahoo! and Google for the executive's last name, last name with first initial, and last name and former-firm name. In cases where we found information about the executive's subsequent employment, we read through the disclosed biographies and years of employment to verify this was the displaced executive from our sample.

Consistent with the general entrenchment hypothesis, we find that in cases of accounting irregularities, founder CEOs are much less likely to be fired than non-founder CEOs. We also find evidence consistent with founder firms designating the CFO as the "scapegoat" for the accounting irregularity. Furthermore, we find evidence that shifting blame away from the CEO and toward the CFO is no more likely to occur after accounting irregularities, unless the CEO is a founder.

This study adds to the literature on the board's response to accounting irregularities and management entrenchment. Prior research focuses on how firm performance affects the board's CEO turnover decision. However, as Brickley (2003) emphasizes, the economic significance of the relation between CEO turnover and firm performance is generally quite small in magnitude. In contrast, the announcement of an accounting irregularity is well known to have a large impact on the probability of CEO turnover. This feature of accounting irregularities makes our setting appealing to test whether the board's response is influenced by the characteristics of the manager. Our findings suggest both a statistically and economically significant effect of CEO entrenchment on executive turnover decisions in the context of accounting irregularities.

Our findings also suggest that more research is warranted into the nature and level of independence involved in the investigatory process around accounting irregularities. The vast majority of the firms in our sample announced an internal investigation into the alleged accounting irregularities. Nonetheless, our evidence suggests that these board-initiated investigations may lack objectivity when the CEO in question is also the founder of the firm.

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Earnings Smoothness, Average Returns, and Implied Cost of Equity Capital

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ABSTRACT: Despite a belief among corporate executives that smooth earnings paths lead to a lower cost of equity capital, I find no relation between earnings smoothness and average stock returns over the last 30 years. In other words, owners of firms with volatile earnings are not compensated with higher returns, as one would expect if volatile earnings lead to greater risk exposure. Although prior empirical work links smoother earnings to a lower implied cost of capital, I offer evidence that this link is driven primarily by optimism in analysts' long-term earnings forecasts. This optimism yields target prices and implied cost of capital estimates that are systematically too high for firms with volatile earnings. Overall, the evidence is inconsistent with the notion that attempts to smooth earnings can lead to a lower cost of equity capital.

Keywords: *cost of capital; earnings smoothness; asset pricing; analyst forecasts.*

Data Availability: *Data are available from commercial vendors.*

I. INTRODUCTION

In an influential survey article, Graham et al. (2005) report that corporate executives express a strong desire to report smooth earnings paths, holding cash flow volatility constant. Surprisingly, executives also indicate a willingness to sacrifice long-term value to achieve smoother earnings. A primary motivation offered for such behavior is that executives believe that investors perceive firms with smoother earnings to be less risky, and thus demand a lower expected return, or cost of equity capital. In this study, I use asset-pricing tests—common in financial economics—to assess the relation, if any, between earnings smoothness and expected returns.

Though prior empirical work in accounting has linked smoother earnings to a lower implied cost of equity capital (Francis et al. 2004; Verdi 2006), I re-examine the issue for three reasons. First, theoretical support for the notion that smoother earnings lead to a lower

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cost of equity capital is mixed. Second, inferences in prior research are based primarily on an association between earnings smoothness and cost of capital estimates imputed from Value Line analysts' forecasted target prices. In short, these studies find that analysts *expect* firms with smoother earnings to experience lower future returns than firms with volatile earnings. Given the importance of the research question, it is critical to seek corroborative evidence from alternative methodologies. Most asset-pricing studies in financial economics evaluate whether a variable is linked to expected returns by examining its relation with average realized stock returns. I use asset-pricing regressions to test whether firms with smoother earnings do in fact experience actual lower future returns. In this manner, I can benchmark analysts' expectations of future share price appreciation against realized outcomes in the equity market.

Finally, Core et al. (2008) subject another accounting attribute—accrual quality—to asset-pricing tests and find no evidence that is a separately priced "risk factor," even though implied cost of capital tests indicate it is. This study investigates whether a similar pattern exists for earnings smoothness and, if so, seeks to explain why different methodologies yield different results.

My empirical tests yield the following findings. First, I find no relation between earnings smoothness (defined as earnings volatility relative to cash flow volatility) and average stock returns from 1975 to 2006. Earnings smoothness, in isolation or combined with other risk proxies, has no ability to explain average returns, either at the firm or the portfolio level. Thus, there is no evidence that owners of firms with volatile earnings are rewarded with higher average stock returns.

Second, I offer evidence that the link between implied cost of equity capital and earnings smoothness results primarily from optimistic bias in analysts' long-term earnings projections. Specifically, I document a positive association between optimism in analysts' earnings forecasts and earnings volatility. This optimistic bias in forecasted earnings yields projected future stock prices, and thus implied cost of equity capital estimates, that are systematically too high for firms with volatile earnings. Finally, I show that a substantial portion of the relation between optimism in analysts' earnings forecasts and earnings volatility can be explained by analysts' mis-weighting of prior earnings changes when predicting future earnings changes.

This study contributes to the extant literature in at least three ways. First, the asset-pricing evidence does not support the apparently widely held belief among corporate executives that smoother earnings can lower cost of equity capital. This study is the first of which I am aware to examine smoothness and average stock returns, and its findings should interest managers, investors, and researchers. Second, this study is the first to reconcile differences in results from asset-pricing and implied cost of capital tests. Specifically, the evidence helps explain why implied cost of capital estimates based on projected target prices are correlated with earnings smoothness, but actual stock returns are not. This analysis is important not just for the present study, but may also explain why a similar phenomenon is observed with other accounting attributes, such as accrual quality (Core et al. 2008). Finally, this study contributes to a growing literature that raises concerns over cross-sectional inferences from implied cost of capital tests (Easton and Sommers 2007; Ogneva et al. 2007). Findings highlight the importance of ensuring that the association between implied cost of equity capital and the variable of interest is not driven by biased input variables (e.g., earnings forecasts).

These findings are subject to two important caveats. First, I use *ex post*, realized stock returns to proxy for expected returns in the asset-pricing analysis. In addition to expected returns, realized returns are comprised of information surprises (news about future cash flows or discount rates). In supplementary analysis, I find no evidence that information surprises compromise inferences from the asset-pricing tests. Nevertheless, since true information surprises are unobservable, I cannot completely dismiss them as a confounding factor in the analysis. Thus, while the asset-pricing tests are inconsistent with a relation between smoothness and cost of equity capital, they do not necessarily offer unequivocal evidence that there is no relation. Second, tests that examine the link between earnings smoothness and cost of equity capital are actually joint tests of two hypotheses: (1) smoother earnings reduce some form of information risk, and (2) this information risk is priced. Failure to document a link between average returns and earnings smoothness casts doubt on at least one hypothesis, but it does not provide evidence as to which hypothesis may be incorrect.

II. PRIOR LITERATURE

The literature on earnings smoothing is vast, dating back to the 1960s. I briefly review only two strands of this literature. Since the extent to which one believes smoother earnings can reduce cost of capital often hinges on normative judgments, I discuss this area of the literature first. Second, I discuss analytical and empirical work that links smoother earnings to cost of capital.

Smooth Earnings: Benefits and Drawbacks

Disagreement exists in the literature as to whether smoothness is a desirable property of accounting earnings. Barnea et al. (1975) argue that smoother earnings allow outsiders to better predict future earnings, and Chaney and Lewis (1995) construct a model based on Spence's (1973) signaling theory in which income smoothing is used by "high-quality" firms to signal their type. Beidleman (1973) asserts that smoothing is useful for internal budgeting and in reducing perceived riskiness among outsiders. Other authors, however, cast earnings smoothing in a less favorable light. Leuz et al. (2003) view earnings smoothing as a device used by insiders to obfuscate their consumption of private control benefits. Bhattacharya et al. (2003) contend that smoothing leads to greater earnings "opacity," while Myers et al. (2007) offer evidence that firms use income smoothing as an earnings management tool to maintain artificially long strings of increasing EPS.

Earnings Smoothing and Cost of Capital—Analytical Work

In traditional asset-pricing theory, only systematic, undiversifiable risks affect expected returns. This framework casts doubt on any association between firm-specific earnings volatility and cost of capital.[1] In the CAPM, for example, idiosyncratic return volatility is not priced because it does not affect the variance of a well-diversified portfolio. Similar logic can be applied to earnings volatility. By holding a diversified portfolio of stocks, investors can lay claim to a stream of earnings that is much "smoother" that any of its components. In short, portfolio theory suggests firms need not smooth out idiosyncratic "blips" in their earnings, since investors can do this themselves.

Some studies, however, argue for a link between earnings smoothness and cost of capital. Trueman and Titman (1988) develop a model in which firms smooth earnings to reduce their perceived probability of bankruptcy, thereby lowering borrowing rates. The model does not address cost of equity capital directly, however, and does not allow for investor diversification across multiple securities as in the CAPM. Easley and O'Hara (2004) construct a model in which uninformed investors demand a risk premium for securities when they lack information *vis-à-vis* informed investors. The authors argue this "information risk" is undiversifiable. So, to the extent income smoothing reduces information asymmetry, smoother earnings may be associated with a lower cost of equity capital. However, given the above discussion, the effect of earnings smoothness on information asymmetry remains unclear. If smoothness facilitates the predictability of future earnings or cash flows, or helps signal the underlying economic "type" of a firm, information asymmetry could be attenuated. On the other hand, if earnings smoothness plays primarily an obfuscatory role that leads to greater reporting opacity, information asymmetry could be exacerbated.

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[1] I focus on cost of equity capital only, not cost of debt capital or total cost of capital. For convenience, I use the terms cost of equity capital and cost of capital interchangeably.

Furthermore, recent research by Lambert et al. (2008, 2007) challenges the notion that information risk, as modeled by Easley and O'Hara (2004), is even priced in equilibrium, suggesting instead that it can be diversified away by investors. They contend that better quality financial reporting (which smoothing, for the reasons listed above, may or may not facilitate) can directly reduce cost of capital, but only through a firm's CAPM beta.

Finally, Goel and Thakor (2003) model a setting in which earnings volatility benefits informed investors at the expense of uninformed investors, because the latter demand a risk premium to stay in the market. Firms respond by smoothing earnings to reduce perceived volatility, but these smoothing attempts are unwound by the market due to rational expectations and have no effect on cost of capital in equilibrium.

Earnings Smoothing and Cost of Capital—Empirical Evidence

The most direct empirical evidence of a link between cost of capital and earnings smoothness comes from Francis et al. (2004). They examine seven accounting attributes, one of which is earnings smoothness, and find a reliably negative association between the smoothness of earnings and implied cost of capital estimates based on forecasts of future stock prices and dividends provided by Value Line. In essence, the findings of Francis et al. (2004) indicate Value Line analysts *expect* firms with volatile earnings to earn higher future stock returns than firms with smooth earnings. As a robustness check, Francis et al. (2004) document a negative relation between earnings smoothness and the earnings-based MPEG cost of capital estimate derived in Easton (2004). They also offer evidence that earnings smoothness (along with other attributes) serves as the basis for a mimicking-portfolio risk factor that is priced in equilibrium, but Core et al. (2008) object to their methodology. I discuss this issue in more detail in the next section.

Verdi (2006) extends Francis et al. (2004) using principal components analysis and replicates their main results using Value Line-based cost of capital estimates. However, the relation between cost of capital and earnings smoothness reverses in sign and becomes insignificant when he uses a cost of capital estimate derived by Gebhardt et al. (2001). Finally, Bhattacharya et al. (2003) relate three country-level measures of earnings manipulation, one of which is earnings smoothing, to country-level cost of capital measures. Using cost of capital estimates from dividend yields, they find that countries with smoother earnings have a *higher* cost of capital, but this relation becomes insignificant when they use an international factor-pricing model to measure cost of capital. Since earnings smoothness *per se* is not the focus of their study, Bhattacharya et al. (2003) offer no reasons for their differing results.[2]

In summary, the literature on earnings smoothness does not yield a consensus as to whether it (1) represents a beneficial or desirable attribute of financial reporting, or more importantly (2) affects cost of equity capital. Moreover, there is no evidence as to whether earnings smoothness is related to average realized stock returns at the firm or portfolio level, which is a common test used in much of financial economics to judge candidate risk factors. The next section explains how the current study can help to fill this void in the literature.

III. ASSET-PRICING TESTS

To test the hypothesis that smoother earnings are associated with a lower cost of equity capital, I first employ standard asset-pricing tests that relate earnings smoothness to average realized stock returns. Realized returns are, tautologically, a function of two things: expected returns and information surprises (Campbell 1991). The expected return component derives from the discount rate (i.e., cost of capital) investors apply to a stock's expected payoffs at the beginning of the return period. Information surprises represent changes in investors' expectations of future payoffs and required discount rates during the return period. Assuming rational pricing, information surprises are zero in expectation and unpredictable through time. Thus, average realized returns, across firms and over time, proxy for expected returns.

While this methodology has been standard practice in finance over the last 30 years, it has two potential shortcomings. First, even if information surprises are mean zero, they can have substantial variance, particularly at the individual-stock level. This adds noise to the analysis and could lead to low power tests (Botosan and Plumlee 2005). I address this concern in the next section by grouping stocks into portfolios to help mitigate the effects of firm-specific noise. Second, information surprises may not be mean zero over the sample period, yielding a biased proxy for expected returns (Elton 1999). Fama and French (2002), for example, offer evidence that the average return on the S&P index was "a lot higher than expected" over the last 50 years, due to discount rate shocks. However, even if the average return across all stocks is higher than expected over the sample period, inferences from cross-sectional asset-pricing tests are still valid. As Francis et al. (2004, 1002) note, bias in realized returns will confound inferences in asset-pricing tests only if it is correlated cross-sectionally with variable of interest. I discuss this issue in more detail in Section V.

Data

Data for the asset-pricing tests come from the intersection of the CRSP Monthly Stock File and the Compustat Industrial Annual File, obtained through Wharton Research Data Services (WRDS). The sample period runs from 1/1/75 to 12/31/06. I begin in 1975 because this marks the first year that Value Line implied cost of capital estimates can be obtained (Brav et al. 2005; Francis et al. 2004). To remain in the sample, I require a firm-month to have non-missing return data on CRSP and non-missing values for the Compustat accounting variables described below. In total, the asset-pricing sample contains 682,435 firm-month observations for 6,076 unique firms.

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[2] Other studies, such as Hunt et al. (2000) and Thomas and Zhang (2006), relate earnings smoothness to price multiples such P/E. While these studies can speak to the valuation implications of smooth earnings in general, they cannot speak to cost of capital effects directly, since discount rate and expected cash flow effects are difficult (if not impossible) to disentangle using price multiples.

Empirical Methodology and Results

To investigate whether more volatile earnings are associated with higher stock returns, I first run the following cross-sectional regression every month from 1/1/75 to 12/31/06:

$$R_i - R^f = \alpha + \beta_1 Smooth_i + \beta_2 Beta_i + \beta_3 Size_i + \beta_4 BM_i + \varepsilon_i. \tag{1}$$

R_i is the raw monthly stock return for firm i, stated as a percentage. R^f is the return on the one-month T-bill obtained from WRDS. *Smooth* is the decile rank of a firm-year's earnings smoothness measure. I measure earnings smoothness as the standard deviation of net income (scaled by average total assets) divided by the standard deviation of cash flows from operations (scaled by average total assets). Thus, higher values of this variable indicate more earnings volatility. Scaling by cash flow volatility measures the extent to which accrual accounting has smoothed out the underlying volatility of the firm's operations. The smoothness measure is calculated at the annual level over rolling ten-year windows ending in the current fiscal year.[3] I define earnings smoothness in this manner to be consistent with prior research (Francis et al. 2004; Leuz et al. 2003) and because it corresponds closely to the construct described by executives in the Graham et al. (2005) survey.

I also include other standard factors in the model. *Beta* is the slope coefficient from the regression of a firm's monthly raw returns on the monthly value-weighted market return over a rolling five-year window ending in the current fiscal year. *Size* is the natural log of market value of equity, measured at the end of the current fiscal year. *BM* is the natural log of the ratio of book value of equity to market value of equity, measured at the end of the current fiscal year. All variables for a given fiscal year become available for the monthly regressions four months after the fiscal year-end. For example, a December-year-end firm gets a new smoothness, *Beta*, *Size*, and *BM* measure the following April.[4]

Panel A of Table 1 reports time-series averages of the parameters from the 384 monthly regressions in Equation (1). Standard errors are calculated from the time-series variation in these parameters. This methodology, developed by Fama and MacBeth (1973), ensures the reported t-statistics account for cross-sectional correlation in the error terms in Equation (1). Estimates from Table 1 indicate that earnings smoothness is not associated with average realized stock returns, either in isolation ($t = 0.70$) or in conjunction with *Beta*, *Size*, and *BM* ($t = 0.47$). Also, *Beta* is not associated with average returns, while *Size* (*BM*) is negatively (positively) related to average returns over the sample period, consistent with extant work in financial economics (e.g., Fama and French 1992). Results are similar if the raw, rather than the ranked, value of earnings smoothness is used instead.

Panels B and C of Table 1 report descriptive statistics along with univariate correlations between *Smooth* and the other covariates in Equation (1). Firms with volatile earnings tend to be smaller, have higher betas, and lower book-to-market ratios than firms with smooth earnings. The correlations are modest, however, so earnings smoothness does not appear to be "swamped" by the other covariates. In short, cross-sectional tests provide no support for the notion that stockholders of firms with volatile earnings are compensated with higher average returns.

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[3] Requiring a ten-year window to estimate earnings smoothness restricts the sample to typically larger, more successful firms, as Francis et al. (2004) note, and limits the extent to which the findings can be generalized. However, notions of a "smooth" or "volatile" earnings stream lose meaning in the absence of a sufficient time-series.

[4] I provide further variable construction details in the table footnotes.

TABLE 1
Cross-Sectional Regression of Monthly Returns on Earnings Smoothness

Panel A: Regression Results

	Intercept	***Smooth***	***Beta***	***Size***	***BM***	**R²**
Estimate	**1.15**	0.013				0.005
t-stat	(5.15)	(0.70)				
Estimate	**1.841**	0.006	0.076	**−0.132**	**0.241**	0.035
t-stat	(6.72)	(0.47)	(0.51)	(−3.06)	(3.62)	

Panel B: Descriptive Statistics for Full Sample

Variable	**Mean**	**5th**	**25th**	**Median**	**75th**	**95th**
Smoothness Measure	0.652	0.178	0.369	0.592	0.878	1.299
Beta	1.016	0.112	0.607	0.981	1.356	2.091
Size	5.184	1.754	3.520	5.068	6.745	8.974
BM	−0.415	−1.724	−0.860	−0.376	0.084	0.781
Ret	1.122	−17.660	−5.653	0.094	6.599	21.868

Panel C: Univariate Correlations with Smoothness Measure

Beta	***Size***	***BM***
0.155	−0.106	−0.029

For each month, from 1/1/75 to 12/31/06, the cross-section of excess realized returns is regressed on the variables listed above. Results are reported in Panel A. Monthly excess returns are measured in percentages and are calculated as the raw stock return less the risk-free rate. The raw stock return is obtained from the CRSP monthly stock file and the risk-free rate is the return on the one-month T-bill obtained from the Fama-French files at WRDS. *Smooth* is the decile rank of a firm-years' smoothness measure, which is calculated from the Compustat Industrial Annual File as the time-series standard deviation of net income before extraordinary items (#18) (scaled by average total assets (#6)) divided by the standard deviation of cash flows (scaled by average total assets) over the ten previous years ending in the current fiscal year. Cash flows equal net income less accruals. Accruals are the change in current assets (#4) minus the change in cash (#1) minus the change in current liabilities (#5) plus the change in ST debt (#34) minus depreciation (#14), scaled by average total assets. The smoothness measure is calculated at the annual level over rolling ten-year windows ending in the current fiscal year. Decile ranking is performed monthly. *Beta* is the slope coefficient from the regression of a firm's monthly raw returns on the monthly value-weighted market return (from the CRSP monthly index file) over a rolling five-year window ending in the current fiscal year. I require at least 18 monthly returns over the rolling five-year interval to calculate *Beta*. *Size* is the natural log of market value of equity from Compustat (#25 * #199), measured at the end of the current fiscal year. *BM* is the natural log of the ratio of book value of equity to market value of equity (log(#60) less *Size*), measured at the end of the current fiscal year. All variables for a given fiscal year become available for the monthly regressions four months after the fiscal year-end. For example, a December year-end firm gets a new *Smooth, Beta, Size,* and *BM* measure each April. Parameter estimates are time-series averages of the parameters from the 384 monthly cross-sectional regressions. t-statistics are calculated from the standard errors of these monthly averages. Panel B contains descriptive statistics and Panel C contains univariate correlations between *Smooth* and *Beta, Size,* and *BM.* Throughout the table, parameter estimates significant at the 5 percent level or lower (two-tailed) are bolded.

One criticism of these cross-sectional tests is that realized returns are noisy, particularly at the firm level. That is, firm-specific news may be so vast as to swamp any pattern that exists in realized returns related to earnings smoothness. To deal with this concern, I perform portfolio time-series regressions in the tradition of Black et al. (1972). Each month, from 1/1/75 to 12/31/06, I group firms into ten portfolios based upon the latest decile rank of their earnings smoothness measure, estimating the following time-series regression for each portfolio:

$$R_{pt} - R_t^f = \alpha + \beta_1(R^M - R^f)_t + \beta_2 SMB_t + \beta_3 HML_t + \varepsilon_i. \tag{2}$$

R_{pt} is the value-weighted return on portfolio p ($p = 1$ through 10) for month t. $R^M - R^f$ is the monthly excess return on the value-weighted market return from CRSP. *SMB* (Small minus Big) is the excess monthly return of small firms over big firms, and *HML* (High minus Low) is the excess monthly return of high *BM* firms over low *BM* firms. *SMB* and *HML* were created by Fama and French (1993), who argue that the slopes on these variables in Equation (2) proxy for an asset's exposure to underlying risk factors—such a shifts in investors' future investment opportunities (Merton 1973)—that is compensated in average returns. This claim remains controversial, however.[5]

Regardless of whether the reader believes Equation (2) captures a rational, equilibrium pricing model, it is useful in this study for two reasons. First, grouping firms into portfolios reduces a substantial portion of the idiosyncratic variation in returns observed at the firm level. For example, the average portfolio time-series standard deviation of excess returns in my sample is 67 percent lower than the average firm-level standard deviation of returns (untabulated). Thus, utilizing portfolios reduces a considerable amount of the firm-specific "noise" in realized returns and produces a more stable return variable from which to identify any pattern related to earnings smoothness.

Second, if earnings smoothness is indeed a "risk factor" compensated in average returns, and this factor is orthogonal to any *Beta*, *Size*, or *BM* effects, then one should observe generally increasing estimates of α in Equation (2) as the portfolios progress from smooth to volatile earnings. The reason is that α represents the portion of an asset's average return in excess of that predicted by the asset's sensitivity to the risk factors in the model. If a factor model is properly specified (that is, if there are no omitted risk factors), then the estimated αs should be zero (Black et al. 1972; Cochrane 2001, Chap. 12). If, however, the model omits a risk factor, then portfolios with greater exposure to that factor will have a greater portion of average excess returns left unexplained (i.e., a higher α).

Panel A of Table 2 presents the results of the ten time-series portfolio regressions, along with an additional hedge portfolio regression. The hedge portfolio goes long (short) in portfolio 10 (1), and measures the monthly difference in returns between firms with the most volatile earnings and firms with the smoothest earnings. In Model 1, I include only an intercept to provide a sense of the average realized monthly return for each portfolio. There does not appear to be a clear, increasing trend in average returns from the smoothest to the most volatile earnings portfolios. Thus, there does not appear to be a link between earnings smoothness and average returns, even on a univariate basis. The hedge portfolio intercept indicates that firms in the most volatile earnings decile earn about six basis points more a month in excess returns than firms in the smoothest earnings decile, but this difference is not statistically different from zero ($t = 0.38$).

Model 2 includes all of the variables from Equation (2). Now, none of the intercepts are significantly different from zero at the 5 percent level (two-sided). The hedge portfolio actually earns an average negative monthly return, inconsistent with the notion that more volatile earnings lead to a higher risk premium, but this difference is insignificant as well ($t = -0.71$). The nearly monotonic loadings on $R^M - R^f$, *SMB*, and *HML* across portfolios

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[5] Some authors, for example, claim the ability of *BM* to predict future returns is related not to sensitivity to some underlying risk factor, but rather to investor irrationality (see Lakonishok et al. 1994).

TABLE 2
Time-Series Regressions of Monthly Portfolio Returns on the Fama-French Three Factors

Panel A: Portfolio Regressions

	Smoothest Earnings							Most Volatile Earnings			Hedge
Portfolio Number	**1**	**2**	**3**	**4**	**5**	**6**	**7**	**8**	**9**	**10**	**10 − 1**
Model 1											
Intercept (α)	**1.012**	**1.014**	**1.064**	**1.108**	**1.074**	**0.969**	**1.023**	**1.057**	**1.040**	**1.074**	0.062
t-stat	**(4.53)**	**(4.58)**	**(4.59)**	**(4.62)**	**(4.51)**	**(3.98)**	**(4.01)**	**(4.02)**	**(3.60)**	**(3.43)**	(0.38)
Model 2											
Intercept (α)	0.074	0.094	0.107	0.139	0.101	0.011	0.066	0.089	0.038	−0.019	−0.092
t-stat	(0.93)	(1.26)	(1.45)	(1.91)	(1.33)	(0.15)	(0.94)	(1.06)	(0.39)	(−0.17)	(−0.71)
$R^M - R^f$	**0.951**	**0.939**	**0.969**	**0.993**	**0.995**	**1.006**	**1.027**	**1.012**	**1.078**	**1.140**	**0.190**
t-stat	**(49.05)**	**(51.59)**	**(53.9)**	**(56.09)**	**(53.64)**	**(56.62)**	**(60.55)**	**(49.16)**	**(45.32)**	**(42.11)**	**(5.97)**
SMB	**0.325**	**0.338**	**0.403**	**0.428**	**0.402**	**0.425**	**0.483**	**0.563**	**0.618**	**0.728**	**0.404**
t-stat	**(12.89)**	**(14.29)**	**(17.25)**	**(18.6)**	**(16.67)**	**(18.4)**	**(21.9)**	**(21.07)**	**(20.02)**	**(20.69)**	**(9.78)**
HML	**0.421**	**0.387**	**0.381**	**0.354**	**0.378**	**0.314**	**0.242**	**0.234**	**0.169**	**0.203**	**−0.218**
t-stat	**(14.71)**	**(14.42)**	**(14.36)**	**(13.52)**	**(13.79)**	**(11.96)**	**(9.65)**	**(7.69)**	**(4.82)**	**(5.09)**	**(−4.64)**
R^2	0.88	0.90	0.91	0.92	0.91	0.92	0.93	0.91	0.90	0.89	0.42

Panel B: Hypothesis Test

GRS Test for Model 2		
Ho	Ha	p
all α = 0	all α ≠ 0	0.24

For each month, from 1/1/75 to 12/31/06, all firm-years in the sample are sorted into ten deciles based upon their earnings smoothness measure. See Table 1 for a description of how earnings smoothness is measured. Portfolios of stocks are then formed for each earnings smoothness decile. Model 1 reports the parameter estimates from a regression of the portfolio excess returns on an intercept only (i.e., the average excess returns for each portfolio over the sample period). Excess returns equal the value-weighted return on the portfolios less the risk-free rate. Model 2 reports the parameter estimates from a regression of the portfolio excess returns on the three Fama-French factors. Parameter estimates significant at the 5 percent level or lower (two-tailed) are bolded. $R^M - R^f$ is excess return on the value-weighted market portfolio. *SML* is the value-weighted size-mimicking portfolio return. *HML* is the value-weighted BM-mimicking portfolio return. R^M, R^f, *SMB*, and *HML* are obtained directly from Wharton Research Data Services via Ken French's website. Monthly returns are measured in percentages. The GRS test (see Gibbons et al. 1989) is a test of whether all intercepts in Model 2 are jointly equal to 0.

are consistent with the univariate correlations from Table 1. Firms with smoother earnings are bigger, have lower CAPM betas, and are more likely to be "value" firms than firms with volatile earnings.[6] Panel B of Table 2 reports the Gibbons et al. (1989) test, which indicates that the null hypothesis that all intercepts in Model 2 are jointly zero cannot be rejected ($p = .24$). This evidence, combined with the fact that all intercepts in Model 2 are individually insignificant and the R^2s are generally at 90 percent or better, indicates that the three-factor model of Fama and French (1993) fares quite well in pricing the test portfolios.

The findings in Tables 1 and 2 are robust to a variety of alternative specifications. These include replacing *Smooth* with earnings volatility not scaled by cash flow volatility, estimating *Smooth* over a shorter window, measuring *Smooth* using quarterly earnings, separating *Smooth* into innate versus discretionary or "managed" components, adjusting *Smooth* for differences across industries, considering the pricing effects of smooth, increasing earnings, backing out one-time special items, controlling for trended earnings growth, using annual cross-section regressions, and using equal weighted returns in the portfolio tests.

As a final test, I examine whether earnings smoothness, as a potential "risk factor," augments the three-factor model in Equation (2). If earnings smoothness is truly a risk factor, then assets that co-vary more strongly with a portfolio designed to mimic exposure to this risk factor should command a higher risk premium and earn higher returns. To this end, for each month I construct a factor-mimicking portfolio called *VMS* (Volatile minus Smooth) by subtracting the value-weighted return of stocks in the lowest three deciles from the value-weighted return on stocks in the highest three deciles of earnings smoothness. I then conduct time-series regressions of the form:

$$R_{it} - R^f_t = \alpha + \beta^{Rm-Rf}(R^M - R^f)_t + \beta^{SMB}SMB_t + \beta^{HML}HML_t + \beta^{VMS}VMS + \varepsilon_i. \tag{3}$$

Panel A of Table 3 contains average parameter estimates from firm-specific regressions across 5,336 firms with at least 18 monthly return observations over the sample period. Standard errors are based on variation in the parameters across firms. This procedure leads to inflated t-statistics since because is likely strong cross-correlation in the slope coefficients across firms. However, I do not draw any inferences from Panel A, so I make no correction for this bias. The results in Panel A are qualitatively similar to those of Francis et al. (2004, Table 10). They interpret the positive and significant loading on *VMS* as indicating that earnings smoothness is a priced risk factor. Core et al. (2008) take issue with this interpretation and argue that a positive and significant loading in Equation (3) is insufficient evidence to indicate that a factor is priced. For example, the loading on the market factor in Panel A is positive and significant; there is strong correlation between individual stock returns and the market portfolio. This correlation does not imply, however, that the CAPM is true and that market risk is priced. The central prediction of the CAPM is that assets with a higher β^{Rm-Rf} should earn a higher return. Analogously, if earnings smoothness is a priced risk factor, then assets with a higher β^{VMS} should earn a higher return.

To test for this relation, I first estimate the factor loadings (i.e., the βs) in Equation (3) at the portfolio level to mitigate the errors-in-variables problem that arises from firm-specific estimation (Black et al. 1972; Fama and MacBeth 1973). Core et al. (2008) use a similar

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[6] In untabulated tests, I regress, at the portfolio level, average (unranked) smoothness measures on average ***Beta***, ***Size***, and ***BM***. The adjusted R^2 is roughly 22 percent, leaving 78 percent of the variation in earnings smoothness across portfolios unexplained. Still, this orthogonal component is not compensated in average returns, as evidenced in Tables 1 and 2.

TABLE 3
Average Returns and Earnings Smoothness "Factor Loadings"

Panel A: Average Firm-Specific Factor Loadings

	Intercept	$R^M - R^f$	*SMB*	*HML*	*VMS*	**R^2**
Estimate	**0.193**	**0.891**	**0.698**	**0.349**	**0.425**	0.24
t-stat	**(5.99)**	**(70.00)**	**(40.54)**	**(19.11)**	**(15.53)**	

Panel B: Average Factor Loadings across 25 Portfolios Sorted on B/M and Smoothness

	Intercept	$R^M - R^f$	*SMB*	*HML*	*VMS*	**R^2**
Estimate	**0.091**	**1.003**	**0.493**	**0.390**	0.088	0.87
t-stat	**(4.06)**	**(123.77)**	**(12.34)**	**(6.04)**	(1.08)	

Panel C: Monthly Cross-Sectional Regressions of Portfolio Returns on Factor Loadings

	Intercept	β_{RM-Rf}	β_{SMB}	β_{HML}	β_{VMS}	**R^2**
Estimate	0.332	0.203	**0.885**	0.337	0.001	0.47
t-stat	(0.57)	(0.36)	**(2.60)**	(1.57)	(0.01)	

Each month, from 1/1/75 to 12/31/06, I construct a factor-mimicking portfolio called *VMS* (Volatile minus Smooth) by subtracting the value-weighted return of stocks in the lowest three deciles from the value-weighted return on stocks in the highest three deciles of earnings smoothness. See Table 1 for a description of how earnings smoothness and stock returns are measured, and definitions of $R^M - R^f$, *SMB*, and *HML*. Panel A presents average coefficients across 5,336 firm-specific, time-series regressions of excess monthly returns on the three Fama-French factors plus *VMS*. t-statistics are calculated from the standard errors of the average parameters. Panel B presents average coefficients across 25 portfolio-specific, time-series regressions of excess value-weighted monthly returns on the three Fama-French factors plus *VMS*. The 25 portfolios are created by sorting stocks into quintiles based on B/M and earnings smoothness each month. t-statistics are calculated from the standard errors of the average parameters. Panel C presents average coefficients from 384 monthly cross-sectional regressions of excess value-weighted portfolio returns on portfolio factor loadings (i.e., the slope coefficients from the regressions in Panel B). t-statistics are calculated from the standard errors of the average monthly parameter estimates. Parameter estimates significant at the 5 percent level or lower (two-tailed) are bolded.

approach. Each month I sort firms (independently) into quintiles based on earnings smoothness and *BM*, forming 25 portfolios from the intersection of these sorts. Sorting on earnings smoothness ensures sufficient variation in the variable of interest among the test assets; sorting on *BM* produces variation in average returns, thereby giving the model in Equation (3) something to explain.

Panel B of Table 3 provides average parameters estimates across 25 portfolio-specific, time-series estimations of Equation (3). Note in this panel that the average loading on *VMS* is now insignificant (t = 1.08). To test whether higher loadings on *VMS* lead to higher returns, I estimate the following cross-sectional regression for every month:

$$R_{pt} - R_t^f = \alpha + \delta_1\beta_p^{Rm-Rf} + \delta_2\beta_p^{SMB} + \delta_3\beta_p^{HML} + \delta_4\beta_p^{VMS} + \varepsilon_i. \qquad (4)$$

In Equation (4), the βs from the time-series portfolio regressions reported in Panel B of Table 3 are used as covariates to explain cross-sectional variation in the 25 portfolio returns. Panel C reports the time-series average of the parameters from the 384 monthly regressions, along with Fama-Macbeth t-statistics. The loading on β_{VMS} is quite small and insignificant (t = 0.01), providing no support for earnings smoothness as an incrementally priced risk factor. The loading on β_{SMB} is positive and significant at the 5 percent level (t = 2.60), while the loading on β_{HML} is positive but insignificant (t = 1.57). One limitation of the analysis in Table 3 is that the portfolio loadings in Panel B are likely measured more noisily than characteristics like *Size*, *BM*, or *Smooth*, such that measurement error could bias the coefficients in Panel C toward zero (note the insignificant loading on β_{HML}). However, it is worth noting that the characteristic-based tests in Tables 1 and 2 do not support earnings smoothness as a priced risk factor either.

In summary, I find no discernable relation between earnings smoothness and average stock returns over the past 30 years. Tables 1 through 3 indicate that earnings smoothness has no ability to explain variation in average returns, either as a characteristic at the firm or portfolio level or as the basis for a mimicking portfolio in a factor model. Why, then, are smoother earnings strongly associated with a lower implied cost of capital using Value Line estimates? One explanation is that Value Line analyst estimates are systematically optimistic, a possibility I explore in the next section. Another possibility is that *ex ante* Value Line estimates are correct on average, but information surprises contaminate the asset-pricing tests over the last 30 years. I discuss this possibility in Section V.

IV. IMPLIED COST OF CAPITAL TESTS

In this section, I examine the Value Line input variables used to generate implied cost of capital estimates. Specifically, I benchmark Value Line projections against actual realizations to see why Value Line analysts expect (implicitly at least) a relation between stock returns and earnings smoothness when none seems to exist over the sample period. I first provide some background on Value Line and briefly discuss data sources. I then lay out the empirical methodology and results.

Background

Value Line is an equity research firm that provides no brokerage or investment banking services. Their primary product is the Investment Survey, which is distributed weekly to subscribers. The Investment Survey contains historical financial information, as well as projections of future items such as stock prices, earnings, and dividends for more than 1,700 firms. Brav et al. (2005) use projected stock price and dividend information from the Value Line Investment Survey to impute cost of capital. I discuss their procedure in more detail below. These estimates from Brav et al. (2005) serve as the dependent variable that Francis et al. (2004) link to earnings smoothness.

Data

The data used in this section come from a variety of sources. For the period 1975–2001, I obtain implied cost of capital estimates directly from Alon Brav's website.[7] For the period 1989–2001, I obtain the Estimates and Projections file directly from Value Line. This data set contains the projections of future stock prices, earnings, and dividends from the Investment Survey that underlie the implied cost of capital estimates of Brav et al. (2005).[8] The intersection of these two data sets comprises the primary sample for this section, which I refer to as the "projections subsample." I also obtain data from the Compustat, CRSP, and I/B/E/S files as I describe below. Overall, the projections subsample contains 9,105 observations for 1,365 firms over the period 1989–2001.

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[7] http://www.duke.edu/~brav/.

[8] Unfortunately, the Estimates and Projections file obtained from Value Line only contains data on firms that Value Line deems as currently active and/or still covers. To mitigate a potential survivorship problem, I hand-collect data from the hard copy Investment Survey for 2,078 firm-years that have *CoC* estimates from Brav et al. (2005) but are not in the Estimates and Projections file over the sample period.

Empirical Methodology and Results

I begin by replicating the main findings of Francis et al. (2004) using the cost of capital estimates from Brav et al. (2005). They estimate cost of capital from the following expression:

$$(1 + CoC)^4 = \frac{TP}{P} + \frac{DIV\left[\frac{(1 + CoC)^4 - (1 + g)^4}{CoC - g}\right]}{P}, \tag{5}$$

where *TP* is the estimate of four-year ahead stock price, *DIV* is the estimate of next year's dividends per share, g is the estimated annual growth rate of dividends per share, and P is the stock price per share nine days before publication date of the analyst's projections in the Investment Survey. Estimated implied cost of capital is the value of *CoC* that equates both sides of Equation (5). Following Francis et al. (2004), I estimate the following cross-sectional regression every year from 1975 to 2001:

$$CoC_i = \alpha + \beta_1 Smooth_i + \beta_2 Beta_i + \beta_3 Size_i + \beta_4 BM_i + \varepsilon_i, \tag{6}$$

where *Smooth* is the annual decile rank of a firm's earnings smoothness measure and *Beta*, *Size*, and *BM* are defined as before. CoC_i is the estimated cost of capital from Equation (5) and is stated as a percentage. Panel A of Table 4 presents the average parameter estimates, along with Fama-Macbeth t-statistics (adjusted with a Newey and West [1987; Newey-West] correction for autocorrelation at four lags), from the annual regressions in Equation (6). I present estimates for the entire period 1975–2001, as well as the period 1989–2001 covered by the projections subsample. The results are very similar to Table 5 in Francis et al. (2004). They report a coefficient of 0.125 on *Smooth*, while I find a coefficient in Panel A of 0.143 ($t = 4.96$) for the full period. Thus, I confirm the primary findings of Francis et al. (2004) regarding earnings smoothness.

To see why firms with volatile earnings have a higher implied *CoC* than firms with smooth earnings, I turn to the projections subsample. Value Line analysts calculate four-year target prices (*TP*) by forecasting future EPS four years beyond the current year and multiplying this figure by a forecasted price-to-earnings (P/E) ratio (Brav et al. 2005; Courteau et al. 2006).[9] The implication of this calculation by Value Line is a point perhaps overlooked by prior implied cost of capital research. Value Line markets the Investment Survey as tool for investors to pick stocks. Firms with high target prices relative to current prices are viewed as "good buys," with high forecasted earnings growth (presumably that the market has not noticed). To the researcher, though, this spread between current price and forecasted price in Equation (5) is treated as a compensation for risk (i.e., a higher expected return or cost of capital). To the extent that Value Line is overly optimistic in their earnings and target price forecasts for some firms, however, the resulting implied cost of capital estimates derived from Equation (5) will be upwardly biased. To test for this bias, I benchmark these forecasted variables against their actual realizations, and relate any differences to earnings smoothness. Specifically, I define the following variables:

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[9] Value Line confirms this methodology on their website. Visit: http://www.valueline.com/ed_vlpage.html and click on item 6. In untabulated analysis, I find that the Pearson (Spearman) in-sample correlation between projected target prices and the product of forecasted EPS and forecasted P/E is 0.93 (0.98).

TABLE 4
Earnings Smoothness and Implied Cost of Capital from Value Line Estimates

Panel A: Cross-Sectional Regressions of Annual Implied Cost of Capital Estimates on Earnings Smoothness

	Intercept	*Smooth*	*Beta*	*Size*	*BM*	R²
Years: 1975–2001						
Estimate	**21.453**	**0.143**	**3.632**	**−0.671**	**2.542**	0.20
t-stat	(12.10)	(4.96)	(6.77)	(−3.34)	(4.60)	
Years: 1989–2001						
Estimate	**20.100**	**0.163**	**3.962**	**−1.113**	**1.793**	0.25
t-stat	(20.66)	(5.04)	(5.93)	(−19.77)	(4.08)	

Panel B: Average Expected and Actual Values for Value Line Input Variables by Earnings Smoothness Decile: 1989–2001

Smoothness Decile	n	Expected Dividends	Expected Price Appreciation	Actual Price Appreciation	Expected Earnings	Actual Earnings	Earnings Forecast Error	Expected P/E Ratio	Actual P/E Ratio	P/E Forecast Error
Smooth			*TP/P*	*AP*	*FEPS*	*AEPS*	*FE^{EPS}*	*FPE*	*APE*	*FE^{PE}*
1	931	0.136	1.641	1.622	0.117	0.079	0.038	14.778	21.382	−6.592
2	918	0.125	1.663	1.616	0.118	0.083	0.035	14.678	19.353	−4.537
3	946	0.116	1.676	1.667	0.118	0.083	0.036	14.881	18.520	−3.517
4	892	0.123	1.647	1.597	0.116	0.078	0.038	15.020	18.764	−3.697
5	915	0.131	1.678	1.585	0.119	0.077	0.042	15.008	19.176	−4.069
6	898	0.114	1.698	1.554	0.120	0.074	0.046	15.040	19.606	−4.459
7	918	0.090	1.754	1.736	0.123	0.078	0.045	15.223	20.208	−4.904
8	902	0.078	1.779	1.648	0.124	0.071	0.053	15.390	18.767	−3.306
9	898	0.097	1.766	1.624	0.123	0.070	0.053	15.820	20.421	−4.582
10	887	0.082	1.860	1.688	0.128	0.061	0.067	15.631	21.316	−5.549

(*continued on next page*)

TABLE 4 (continued)

Panel A presents coefficients estimates from annual cross-sectional regressions of implied cost of capital estimates on the variables listed above. Implied cost of capital estimates are based on projected target prices and dividends from Value Line (see Brav et al. 2005) and are obtained directly from Alon Brav's website. Implied cost of capital estimates are measured in percentages, stated on an annual basis, and are averaged over a 12-month period ending four months after the end of the fiscal year. *Smooth* is the decile rank (sorted annually) of a firm-years' earnings smoothness measure. See Table 1 for details as to the calculation of earnings smoothness, *Beta*, *Size*, and *BM*. The first regression presents parameter estimates from the 27-year period from 1975 to 2001, while the second regression covers the 13-year period from 1989 to 2001. Parameter estimates are time-series averages of the parameters from the annual cross-sectional regressions. t-statistics are calculated from the standard errors of these annual averages, adjusted for a Newey-West correction at four lags. Parameter estimates significant at the 5 percent level or lower (two-tailed) are bolded.

Panel B presents mean values (by earnings smoothness deciles) of the forecasted and realized values of the input variables that underlie the implied cost of capital estimates used in Panel A. Data is obtained from the Value Line Estimates and Projections File for the period 1989–2001. Expected dividends are Value Line analysts' forecasts of future dividends over the four-year forecast horizon, scaled by *P*, the stock price (from CSRP) nine days before the publication date of the analysts' report. See Equation (5) in the study for more details. *TP* is the midpoint of the range of four-year-ahead target stock price forecasted by Value Line. *FEPS* is the forecasted four-year-ahead *EPS*, scaled by *P*. *AEPS* is the actual four-year-ahead earnings (obtained from I/B/E/S), divided by forecasted four-year-ahead shares outstanding, scaled by *P*. FE^{EPS} is *EPS* forecast error, which equals *FEPS* minus *AEPS*. *FPE* is the forecasted four-year-ahead P/E ratio, calculated as $(TP/P)/FEPS$. *APE* is the actual four-year-ahead P/E ratio, calculated as actual four-year-ahead price per share divided by $AEPS * P$. Actual four-year-ahead price is calculated as future total market value of equity four years after the third month subsequent to the end of the current fiscal year, scaled by forecasted four-year-ahead shares outstanding. FE^{PE} is the P/E forecast error and equals *FPE* minus *APE*. *AP* is actual share price appreciation, which equals $APE * AEPS$. All variables from the Estimates and Projections File are averaged over a 12-month period ending four months after the end of the fiscal year.

$FEPS$ = forecasted four-year-ahead EPS, scaled by P, the current stock price;
$AEPS$ = actual four-year-ahead earnings, divided by forecasted four-year-ahead shares outstanding, scaled by P;
FE^{EPS} = EPS forecast error, calculated as $FEPS - AEPS$;
FPE = forecasted four-year-ahead P/E ratio, calculated as $(TP/P)/FEPS$;
APE = actual four-year-ahead P/E ratio, calculated as actual four-year-ahead price per share divided by $AEPS * P$. Actual four-year-ahead price is calculated as future market value of equity four years after the third month subsequent to the end of the current fiscal year, scaled by forecasted four-year-ahead shares outstanding;
FE^{PE} = P/E forecast error, calculated as $FPE - APE$; and
AP = actual price appreciation, equal to $AEPS \times APE$.

Three methodological details merit discussion before proceeding. First, the Estimates and Projections File from Value Line does not report actual annual four year-ahead earnings in a consistent and reliable fashion. As a result, I obtain actual earnings from I/B/E/S. This approach seems reasonable because both I/B/E/S and Value Line exclude from reported earnings nonrecurring items analysts did not intend to forecast.

Second, of the 9,105 observations in the sample, 1,017 do not have available four-year-ahead stock return or earnings data. Excluding these observations from the analysis could induce a potential survivorship bias. Specifically, because non-survivors have relatively poor future returns and earnings prior to their exit from the sample (untabulated), excluding these observations leads to an upward bias in average future returns and profitability. To the extent that firms with more volatile earnings drop out of the sample at a disproportionate rate, this pattern could bias the results in favor of finding a positive relationship between earnings volatility and future returns or earnings.[10] This problem could be particularly acute over a four-year period, especially when benchmarked against asset-pricing tests, which require only all available future monthly returns up to 12 months.

To mitigate this potential bias, I do the following: If a stock stops trading prior to the end of the four-year cumulation window, then I multiply its last available market value of equity by the delisting return (if available) from CRSP.[11] I then reinvest the proceeds in an equally weighted portfolio (rebalanced monthly) of firms in the same earnings smoothness decile. I calculate the portfolio returns excluding dividends, so as to capture only stock price appreciation. The value of the proceeds at the end of the accumulation window serves as the future market value of equity used to calculate *APE* and *AP*. If a firm is missing four-year ahead actual EPS from I/B/E/S, then I impute this value. Specifically, I multiply the last available value of actual EPS by the appropriately compounded earnings growth rate for the firm's earnings smoothness decile. I also conduct the analysis without this survivorship correction (i.e., by just examining four-year survivors) to assess its impact on my findings. I find inferentially similar results, so it appears that survivorship is not an overriding concern in this setting. Nevertheless, the tabled results that follow include this correction, as I feel it is methodologically appropriate.

Third, I winsorize the variables *TP/P*, *FEPS*, *AEPS*, and *APE* at the top and bottom 1 percent of their distributions each year to mitigate the influence of extreme observations.

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[10] The proportion of non-survivors in the smoothest (most volatile) earnings decile is 8.5 (13.5) percent. The correlation between smoothness decile rank and the proportion of non-survivors in each decile is 0.84 ($p < .01$).

[11] Following Sloan (1996), if the delisting return is missing but the delisting code indicates a forced delisting or liquidation, I assume a delisting return of −100 percent.

Brav et al. (2005) winsorize Value Line target prices in generating their *CoC* estimates, so I winsorize *TP* and *FEPS* for consistency. I winsorize *AEPS* and *APE* since outliers in the raw, price scaled EPS distribution produce extreme actual P/E values. Note that winsorizing *AEPS* and *APE* also implicitly winsorizes actual share appreciation, *AP*. Results are inferentially similar if I delete the top and bottom 1 percent of the variables above each year, or if I make no adjustment for outliers.

Panel B of Table 4 presents mean values, by earnings smoothness decile, of the forecasted and actual variables underlying the implied *CoC* estimates. The third column lists the average expected dividend component of implied *CoC* – the second term on the RHS of Equation (5). This value decreases as earnings become more volatile, while the other component of implied *CoC*, expected share price appreciation (*TP/P*), increases almost monotonically. Thus, expected price appreciation alone drives the positive relation between *Smooth* and implied *CoC*. However, while expected price appreciation increases nearly monotonically across smoothness deciles, actual price appreciation (*AP*) is relatively flat, consistent with the asset-pricing tests in the previous section. As a sensitivity check, I repeat the asset-pricing analysis in Tables 1 and 2 using the projections subsample and find similar results for the smoothness measure.

Forecasted EPS (*FEPS*) in Panel B of Table 4 also increases with earnings volatility, while actual EPS (*AEPS*) appears to decrease. The positive gap between *FEPS* and *AEPS* indicates that earnings forecasts are too optimistic, across the board, for all firms. But as earnings become more volatile, the gap grows, and earnings forecast errors (FE^{EPS}) get larger (i.e., more optimistic). Finally, the last three columns in Panel B of Table 4 report mean values of the P/E variables (*FPE*, *APE*, and FE^{PE}) by smoothness decile. While *FPE* increases nearly monotonically with earnings volatility, no clear pattern emerges for *APE* and FE^{PE}.

Table 4 yields two important takeaways. First, the association between *Smooth* and implied *CoC* is driven exclusively by expected share price appreciation (*TP/P*). Second, the relation between *Smooth* and actual price appreciation (*AP*) appears weak, while the relation between *Smooth* and earnings forecast errors (FE^{EPS}) appears quite strong. This evidence implies that optimism in analysts' expectations of future EPS yields target prices that are systematically too high for firms with volatile earnings. These target prices are ultimately not achieved, explaining why firms with volatile earnings have higher implied *CoC*s, but do not earn higher returns, than firms with smooth earnings.

To investigate this possibility further, I first examine whether the univariate patterns in Panel B of Table 4 hold up in a multivariate framework that controls for *Beta*, *Size*, and *BM* effects. To do so, I estimate the following cross-sectional regressions each year:

$$DepVar_i = \alpha + \beta_1 Smooth_i + \beta_2 Beta_i + \beta_3 Size_i + \beta_4 BM_i + \varepsilon_i, \tag{7}$$

where the covariates are defined as before and $DepVar_i$ is, separately, AP_i, FE^{EPS}, and FE^{PE}. Panel A of Table 5 presents the average coefficient estimates from the annual regressions in Equation (7). Due to the overlapping nature of the data (i.e., annual regressions with four-year cumulation windows), all t-statistics in this section are based on Fama-Macbeth standard errors, adjusted for autocorrelation with a Newey-West correction up to four lags.[12]

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[12] General practice for Newey-West estimation is to use a lag length equal to the smallest integer greater than $T^{.25}$ (Greene 2003, 267). With $T = 13$, this implies a lag length of 2. I repeat all of the subsequent analysis using a lag length equal to 2, and a lag length equal to 0 (i.e., no adjustment). I also repeat the analysis using pooled regressions and clustering standard errors on industry and firm. Results are inferentially similar.

TABLE 5
Earnings Smoothness and Input Variables for Value Line Implied Cost of Capital Estimates

Panel A: Annual Cross-Sectional Regressions of Actual Four-Year Price Appreciation and Forecast Errors on Earnings Smoothness

	Dependent Variable		
	Actual Price Appreciation	Earnings Forecast Error	P/E Forecast Error
	AP	FE^{EPS}	FE^{PE}
Smooth	0.003	**0.003**	−0.006
t-stat	(0.45)	**(3.92)**	(−0.05)
Beta	0.235	**0.026**	1.408
t-stat	(1.55)	**(6.98)**	(1.74)
Size	−0.055	**−0.009**	−0.268
t-stat	(−1.67)	**(−5.52)**	(−0.56)
BM	−0.047	**0.020**	**1.182**
t-stat	(−0.60)	**(5.36)**	**(2.03)**

Panel B: Decomposition of the Relation between Earnings Smoothness and Four-Year Expected Share Price Appreciation

	Dependent Variable								
	Expected Price Appreciation	=	Actual Price Appreciation	+	Earnings Bias	+	P/E Bias	+	Interaction
	TP/P	=	AP	+	$FE^{EPS} * FCPE$	+	$FE^{PE} * FCEPS$	+	$-(FE^{EPS} * FE^{PE})$
Smooth	**0.014**		0.003		**0.032**		−0.002		−0.019
t-stat	**(5.40)**		(0.45)		**(4.06)**		(−0.13)		(−1.54)
Beta	**0.357**		0.235		**0.404**		0.04		**−0.322**
t-stat	**(5.18)**		(1.55)		**(4.90)**		(0.34)		**(−3.15)**
Size	**−0.09**		−0.055		**−0.135**		−0.024		**0.125**
t-stat	**(−10.58)**		(−1.67)		**(−5.20)**		(−0.45)		**(3.39)**
BM	**0.110**		−0.047		**0.18**		−0.038		0.015
t-stat	**(3.68)**		(−0.60)		**(3.82)**		(−0.49)		(0.11)

Panel A provides average parameter estimates from annual regressions of four-year actual price appreciation (AP), earnings forecast error (FE^{EPS}), and P/E forecast error (FE^{PE}) on earnings smoothness and control variables. See Table 4 for variable definitions. Panel B provides average parameter estimates from annual regressions of the components of four-year expected price appreciation (TP/P) on earnings smoothness and control variables. These regressions decompose the slope coefficients from the TP regressions into the four components listed above. See the Appendix for details of the decomposition. Parameter estimates are time-series averages of the parameters from the annual cross-sectional regressions over the 13-year period from 1989 to 2001. t-statistics are calculated from the standard errors of these annual averages, adjusted with a Newey-West (1987) correction for autocorrelation up to four lags. Parameter estimates significant at the 5 percent level or lower (two-tailed) are bolded.

The results in Panel A of Table 5 confirm the univariate findings in Table 4. The relation between *Smooth* and actual price appreciation is insignificant (t = 0.45).[13] *Smooth* is strongly correlated with FE^{EPS} (t = 3.92) as are the other covariates, while there is no significant relation between *Smooth* and FE^{PE} (t = −0.05).

To assess the impact of earnings optimism on the relation between *Smooth* and expected share price appreciation (*TP*/*P*) more formally, I decompose *TP*/*P* into four components:

$$\begin{aligned}\frac{TP}{P} = {} & \underbrace{(AP)}_{\text{Actual Price Appreciation}} + \underbrace{(FE^{EPS} * FPE)}_{\text{Earnings Bias}} \\ & + \underbrace{(FE^{PE} * FEPS)}_{\text{P/E Bias}} - \underbrace{(FE^{EPS} * FE^{PE})}_{\text{Interaction Effect}}.\end{aligned} \tag{8}$$

The Appendix explains the derivation of this decomposition. Equation (8) demonstrates that expected price appreciation can be decomposed into actual price appreciation, plus bias in forecasted earnings (ignoring any bias in forecasted P/E), plus bias in forecasted P/E (ignoring any bias in forecasted earnings), minus the interaction of EPS and P/E bias. The interaction term measures the joint effect of EPS and P/E bias and is necessary because the middle two terms ignore error in forecasted P/E and earnings, respectively. The Appendix provides a simple numerical example.

Equation (8) allows me to decompose the relation between *Smooth* and *TP*/*P* into these four components as well, using OLS regression. Specifically, the slope coefficients from regressing *TP*/*P* on the covariates in Equation (7) simply equal the sum of the slope coefficients from four separate regressions with the four components in Equation (8) serving as dependent variables.[14]

So, in the following regression:

$$TP/P_i = \alpha + \beta_1 Smooth_i + \beta_2 Beta_i + \beta_3 Size_i + \beta_4 BM_i + \varepsilon_i, \tag{9}$$

the βs simply equal the sum of the βs from the following four regressions:

$$\begin{aligned} APi_i &= \alpha + \beta_1 Smooth_i + \beta_2 Beta_i + \beta_3 Size_i + \beta_4 BM_i + \varepsilon_i \\ (FE^{EPS} * FPE)_i &= \alpha + \beta_1 Smooth_i + \beta_2 Beta_i + \beta_3 Size_i + \beta_4 BM_i + \varepsilon_i \\ (FE^{PE} * FEPS)_i &= \alpha + \beta_1 Smooth_i + \beta_2 Beta_i + \beta_3 Size_i + \beta_4 BM_i + \varepsilon_i \\ -(FE^{EPS} * FE^{PE})_i &= \alpha + \beta_1 Smooth_i + \beta_2 Beta_i + \beta_3 Size_i + \beta_4 BM_i + \varepsilon_i. \end{aligned} \tag{10}$$

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[13] Surprisingly, in Panel A of Table 5, while *Size* is negatively related to future returns at the 10 percent level (one-sided; t = −1.67) as expected, the loading on *BM* is insignificant (t = −0.60), while the loading on *Beta* is positive and significant at 10 percent on a one-sided basis (t = 1.55). Upon further investigation, three factors explain why these results differ from the asset-pricing tests. First, using an unwinzorized return metric increases the significance on Size. Second, I perform cross-sectional monthly return regressions—using returns without dividends—in Equation (1) for the projections subsample from 1989–2001. I find very similar results to those reported in the Appendix. When I use returns with dividends (as I do in Table 1), results are the same except *Beta* loses significance. Thus, measuring returns with or without dividends affects the loading on *Beta*. Finally, Brav et al. (2005) note that Value Line tends not to cover small, high *BM* firms. This is where the *BM* effect is the strongest (Loughran 1997). When I expand the sample to all firms in the asset-pricing sample over the same time period, *BM* becomes positive and significant.

[14] To see this, let y be a dependent variable that can be decomposed into i components such that $y = \Sigma_i y_i$. The vector of OLS coefficients is then given by: $[X'X]^{-1}X'y = [X'X]^{-1}X'\Sigma_i y_i = \Sigma_i[X'X]^{-1}X'y_i$.

Panel B of Table 5 provides estimates from the five regressions in Equations (9) and (10). The first row indicates that the positive coefficient relating Smooth to *TP/P*, 0.014 (t = 5.40), is driven primarily by the coefficient relating *Smooth* to bias in forecasted EPS, 0.032 (t = 4.06), consistent with Panel A. As earnings become more volatile, analysts' earnings forecasts become more optimistic. Actual price appreciation and bias in P/E forecast play a small role, and both coefficients individually are insignificantly different from zero. The coefficient on the interaction term, −0.019, is insignificant (t = −1.54) at the 5 percent level.[15] Results for the remaining variables in Panel B are generally similar, with earnings bias and, to some extent, the interaction term playing significant roles in relating these variables to expected price appreciation.

In summary, I conclude from Tables 4 and 5 that bias in analysts' long-term earnings forecasts is the main driver of the association between earnings smoothness and implied cost of capital. As earnings become more volatile, earnings projections become more optimistic, producing target prices that are too high and never achieved. Thus, while Value Line analysts may expect firms with volatile earnings to earn higher returns, this future price appreciation does not materialize over the sample period.

Why is Earnings Optimism Increasing in Earnings Volatility?

Prior research finds that analysts mis-weight the persistence of prior earnings changes when predicting future earnings changes. For example, Easterwood and Nutt (1999) demonstrate that analysts' under-react to large prior earnings decreases and over-react to large prior earnings increases, yielding forecasts of future earnings that are optimistic on average. Firms with volatile earnings naturally have larger prior earnings increases and decreases than firms with smooth earnings. Thus, I conjecture that the large prior earnings changes of volatile earnings firms, combined with analysts' general mis-weighting of prior earnings changes, produce optimism in future earnings forecasts that is increasing in earnings volatility.

To test this conjecture, I adopt a framework similar to Easterwood and Nutt (1999). Define $CEPS_{t-5\rightarrow t-1}$ as the change in earnings per share from year t–5 to $t-1$, scaled by price at the end of year $t-1$. Further, let D be an indicator variable equal to 1 if $CEPS_{t-5\rightarrow t-1}$ is negative, and 0 otherwise. Finally, consider the following regression equations:

$$FEPS_{t+4} - AEPS_t = \lambda + \delta_1 D + \delta_2 CEPS_{t-5\rightarrow t-1} + \delta_3(D * CEPS_{t-5\rightarrow t-1}) + \varepsilon; \tag{11}$$

$$AEPS_{t+4} - AEPS_t = \alpha + \beta_1 D + \beta_2 CEPS_{t-5\rightarrow t-1} + \beta_3(D * CEPS_{t-5\rightarrow t-1}) + \tau; \tag{12}$$

$$\begin{aligned} FEPS_{t+4} - AEPS_{t+4} = {} & (\lambda - \alpha) + (\delta_1 - \beta_1)D + (\delta_2 - \beta_2)CEPS_{t-5\rightarrow t-1} \\ & + (\delta_3 - \beta_3)(D * CEPS_{t-5\rightarrow t-1}) + (\varepsilon - \tau). \end{aligned} \tag*{(11) − (12) = (13)}$$

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[15] Since ($FE^{EPS} * FE^{PE}$) is positive (negative) when FE^{PE} and FE^{EPS} have identical (different) signs, the coefficient on the interaction term can be interpreted as the extent to which variation in the independent variables increases or decreases the likelihood that FE^{EPS} and FE^{PE} will share the same sign.

Equation (11) measures the implicit weights, δ_2 and δ_3, that analysts place on positive and negative prior earnings changes when predicting future four-year earnings changes. Equation (12) measures the "true" relation between actual earnings changes and past earnings changes.[16] Equation (13), the difference between Equations (11) and (12), measures the differences in the weights used by analysts to predict future earnings from the empirical weights. Thus, Equation (13) indicates that, by regressing FE^{EPS} on past earnings changes, one can estimate the extent to which analysts mis-weight prior earnings changes when predicting future earnings changes. If analysts over-react to positive prior earnings changes, then $\delta_2 - \beta_2$ will be positive. If analysts under-react to negative prior earnings changes, then the sum of $\delta_2 - \beta_2$ and $\delta_3 - \beta_3$ will be negative.

Panel A of Table 6 reports average parameters from annual estimates of the regressions in Equation (13) for the Value Line sample. I report estimates for the full sample and for firms in the upper (more volatile) and lower (less volatile) three earnings smoothness deciles. Across all specifications, the coefficient on $CEPS_{t-5\rightarrow t-1}$ is significantly positive, and the coefficient on $D * CEPS_{t-5\rightarrow t-1}$ is significantly negative. In addition, the sum of these two coefficients is significantly negative ($p < 0.01$, untabulated). These findings are consistent with those of Easterwood and Nutt (1999): analysts over(under) react to positive (negative) prior earnings changes. Note that this mis-weighting is present in both smooth and volatile earnings firms.

Can such a finding help explain why analysts' long-term earnings forecasts are more optimistic for firms with volatile earnings? To answer this question, I focus on the subsample of observations in the upper (more volatile) and lower (less volatile) three earnings smoothness deciles. I create an indicator variable, *VOL*, equal to 1 if the observation is in the upper, or more volatile, deciles, and 0 if it is in the lower, or smoother, deciles. In Panel B of Table 6, I regress FE^{EPS} on *VOL*, and separately, on *VOL* and the covariates in Equation (13). These regressions estimate the extent to which analysts' mis-weighting of prior earnings changes can explain the difference in mean FE^{EPS} between the firms with the smoothest and most volatile earnings. The first regression indicates that firms in the most volatile earnings deciles have EPS forecast errors that are higher than firms in the smoothest earnings deciles by 2.2 percent ($t = 4.77$) of equity market value. The second regression indicates that this difference shrinks by almost 70 percent, to 0.7 percent, and becomes insignificant ($t = 1.42$) once prior earnings changes are considered. In short, prior positive and negative earnings changes are correlated with both *VOL* and FE^{EPS}, and their mis-weighting by analysts explains an economically and statistically significant fraction of the difference in FE^{EPS} between volatile and smooth earnings firms.

V. WEIGHING THE EVIDENCE

This study provides evidence that: (1) earnings smoothness is not linked to average returns over the last 30 years, and (2) the relationship between earnings smoothness and implied cost of capital from Value Line is driven by forecast errors in future earnings estimates. One interpretation of this evidence is that earnings smoothness is not a factor compensated in equity cost of capital, and the Value Line implied cost of capital estimates are contaminated by analyst optimism. Another possibility is that *ex post* average returns are

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[16] Easterwood and Nutt (1999) actually split prior earnings changes into quartiles and find evidence of analyst mis-weighting in quartiles 1 and 4 (extreme positive or negative changes). In my sample, I find some evidence of mis-weighting even in intermediate quartiles (untabulated). For this reason, and to simplify the analysis, I just examine positive and negative changes in general.

TABLE 6
The Relation between Future Earnings Forecast Errors and Past Earnings Changes

Panel A: Regression of Four-Year Earnings Forecast Error on Positive and Negative Prior Four-Year Earnings Changes

	Intercept	*D*	*CEPS*	*D * CEPS*	R²	n
Full Sample						
Estimate	**0.028**	**0.011**	**0.319**	**−0.549**	0.055	9114
t-stat	**(8.26)**	**(3.48)**	**(6.05)**	**(−7.58)**		
Smooth Earnings Firms						
Estimate	**0.02**	**0.016**	**0.424**	**−0.700**	0.086	2797
t-stat	**(5.24)**	**(3.30)**	**(4.00)**	**(−4.33)**		
Volatile Earnings Firms						
Estimate	**0.032**	0.016	**0.298**	**−0.504**	0.077	2690
t-stat	**(9.97)**	(3.17)	**(4.05)**	**(−7.04)**		

Panel B: The Role of Past Earnings Changes in Explaining the Difference in Average Earnings Forecast Errors between Volatile and Smooth Earnings Firms

	Intercept	*VOL*	*D*	*CEPS*	*D * CEPS*	R²	n
Estimate	**0.036**	**0.022**				0.02	5487
t-stat	**(7.46)**	**(4.77)**					
Estimate	**0.024**	0.007	**0.015**	**0.319**	**−0.545**	0.083	5487
t-stat	**(6.92)**	1.42	**(3.67)**	**(7.05)**	**(−9.13)**		

Panel A presents results from annual regressions of FE^{EPS} on the variables above, using data from the Value Line Estimates and Projections sample in Tables 4 and 5. FE^{EPS} is defined in Table 4. *CEPS* is the change in net income before extraordinary items per Compustat (#data18) from year *t*–5 to year *t*–1, scaled by market value of equity at the end of year *t*–1. *D* is an indicator variable equal to 1 if *CEPS* is negative, and 0 otherwise. Results are presented for the full sample, as well as firms in the lowest three (smoothest) and highest three (most volatile) earnings smoothness deciles. See Table 1 for details on the earnings smoothness measure.

Panel B uses just observations in the upper and lower three earnings smoothness deciles. *VOL* is an indicator variable equal to 1 if an observation is in the highest three (most volatile) earnings smoothness deciles, and 0 if it is in the lowest three (smoothest) earnings smoothness deciles. Parameter estimates are time-series averages of the parameters from the annual cross-sectional regressions over the 13-year period from 1989 to 2001. t-statistics are calculated from the standard errors of these annual averages, adjusted with a Newey-West (1987) correction for autocorrelation up to four lags. Parameter estimates significant at the 5 percent level or lower (two-tailed) are bolded.

confounded by information surprises over the sample period. Note that the information surprises would have to be correlated in the cross-section with earnings smoothness (Francis et al. 2004). Perhaps, due to chance, firms with more volatile earnings had a disproportionate run of "bad luck" over the sample period, reporting lower earnings (i.e., negative "cash flow" surprises) and thus realizing lower stock returns than analysts and investors were expecting. This possibility would also explain why Value Line analysts appeared, *ex post*, to be overly optimistic in their earnings projections.

I conduct two different tests to evaluate the effects of cash flow surprises on the asset-pricing tests. For brevity, I do not table them here (the results are available upon request). First, I include a future earnings surprise measure as a control variable in the asset-pricing regression from Table 1, following the methodology of Ogneva (2009), who argues that information surprises confound the analysis in Core et al. (2008). The future earnings surprise is measured as actual earnings minus expected earnings, where the latter is based on a statistical model, with prior-year stock returns and earnings as predictor variables.

Second, I exclude all return-months in which a firm announced future quarterly earnings from the asset-pricing tests. By excluding future announcement months, I decrease the likelihood that future information surprises related to earnings news bias the statistical tests. The results of these tests are inferentially similar to those reported in Tables 1 and 2. Unfortunately, neither of these tests is definitive. The statistical model may measure cash flow surprises with error. In addition, cash flow shocks or discount rate shocks could contaminate realized returns in periods other than earnings announcement months. In short, because investors' expectations are unobservable, true information surprises can never be precisely measured. Thus, while I find no evidence that information surprises compromise inferences from the asset-pricing tests, I cannot completely dismiss them as a confounding factor.

As to the reasonableness of Value Line analyst earnings projections, I find in untabulated results that firms with more volatile earnings have relatively lower average *current* earnings in addition to the lower average future earnings documented in Table 4, Panel B. This pattern of relatively low current earnings, relatively low future earnings, yet relatively high future earnings forecasts for firms with volatile earnings (see Table 4, Panel B) seems more consistent with undue optimism in analyst forecasts than with a chance result driven by unexpected shocks to future earnings. The mis-weighting evidence in Table 6 supports this conclusion as well.

In sum, the asset-pricing evidence indicates there has been no return premium to holding volatile earnings stocks over the last 30 years. The implied cost of capital tests indicate that analyst optimism creates a mechanical link between earnings volatility and implied cost of equity capital. The inferences drawn from this evidence likely hinge on the reader's prior beliefs as to the efficacy of asset-pricing tests. If one is suspicious that information surprises during the sample period compromise asset-pricing tests, then evidence that analyst bias contaminates implied cost of capital measures need not imply that earnings smoothness has no bearing on cost of equity capital. If, however, the reader believes that average returns over the sample period are a reasonable proxy for expected returns, the evidence casts doubt on the notion that smoothness is a priced source of risk.

VI. ACCRUAL QUALITY AND ANALYST OPTIMISM

In Table 7, I provide evidence that accrual quality—the residual variance from a regression of accruals on temporally adjacent cash flows—is systematically related to optimism in Value Line's long-term earnings forecasts. Panel A demonstrates that earnings forecast errors increase monotonically across accrual quality deciles (i.e., as accrual quality gets worse), and Panel B indicates that this relation continues to hold after controlling for *Beta*, *Size*, and *BM*. Given that Francis et al. (2004) document that accrual quality and earnings smoothness are highly correlated constructs, this result is perhaps not surprising. However, it does shed light on a puzzle in the literature. Core et al. (2008) find no evidence that accrual quality is a priced risk factor, even though this variable is strongly related to implied cost of capital using Value Line estimates. Table 7 suggests that optimism in Value Line's earnings projections may drive this relation, just as it drives the relation between earnings smoothness and implied cost of capital.

VII. CONCLUSION

The projected target prices of Value Line analysts indicate that they expect firms with volatile earnings to experience greater future stock price appreciation than firms with smooth earnings, creating a negative relation between imputed cost of capital and earnings smoothness. Evidence from the U.S. stock market indicates no such pattern, however. I find

TABLE 7
Accrual Quality and Value Line Four-Year Earnings Forecast Errors

Panel A: Average Four-Year Earnings Expectations versus Actual Earnings by Accrual Quality Decile

Accrual Quality Decile	n	Expected Earnings	Actual Earnings	Earnings Forecast Error
Smooth		*FEPS*	*AEPS*	FE^{EPS}
1	909	0.099	0.087	0.012
2	903	0.105	0.078	0.027
3	886	0.112	0.075	0.037
4	899	0.118	0.085	0.033
5	887	0.122	0.083	0.040
6	896	0.122	0.078	0.044
7	890	0.125	0.077	0.048
8	865	0.130	0.074	0.057
9	866	0.135	0.060	0.075
10	826	0.141	0.059	0.082

Panel B: Annual Cross-Sectional Regressions of Earnings Forecast Error on Accrual Quality (*AQ*)

	AQ	*Beta*	*Size*	*BM*
Estimate	**0.005**	**0.016**	**−0.005**	**0.025**
t-stat	**(8.61)**	**(6.01)**	**(−3.10)**	**(6.11)**

Panel A lists mean values of expected earnings, actual earnings, and forecast errors by accrual quality deciles for the Value Line Estimates and Projections File from 1989–2001. Accrual quality is estimated as the residual variance from firm-level regressions of working capital accruals (accruals plus depreciation) on current, one-year back, and one-year ahead operating cash flows. Accruals and cash flows are defined in Table 1. See Table 4 for details on the additional variables.

Panel B contains average parameter estimates from annual cross-sectional regressions of earnings forecast error (FE^{EPS}) on the decile ranking of accrual quality (*AQ*), as well as *Beta*, *Size* and *BM*. See Table 1 for details as to the construction of the latter three variables. t-statistics are calculated from the standard errors of annual averages, adjusted with a Newey-West (1987) correction for autocorrelation up to four lags. Parameter estimates significant at the 5 percent level or lower (two-tailed) are bolded.

no relation between earnings smoothness and average stock returns over the last 30 years. I offer evidence that the inverse relation between earnings smoothness and implied cost of capital results primarily from optimistic bias in analysts' long-term earnings projections. Specifically, a positive correlation between earnings volatility and earnings optimism yields target prices, and thus implied cost of capital estimates, that are too high for firms with volatile earnings. These findings are important because they call into question the wisdom of smoothing earnings to achieve a lower risk premium, particularly if such actions sacrifice economic value. While smoother earnings may have other benefits, I find no evidence that smoother earnings are associated with a lower return premium. This study also highlights the need to control for biased input variables in studies that use implicit cost of capital estimates, which should be of interest to a variety of researchers.

A number of recent studies investigate the reliability of implied cost of capital estimates (Botosan and Plumlee 2005; Easton and Monahan 2005; Guay et al. 2006). While this study contributes to that literature, I view the evidence neither as a general indictment of implied cost of capital tests nor as a general endorsement of asset-pricing tests. The appropriate methodology for a given study will obviously vary with the research question and setting. In some studies, for example, due to limited sample sizes or data availability, asset-pricing tests may not be feasible. In general, however, when both methodologies are available, the desire for convergent evidence across differing methodologies provides a justification for using both (Campbell and Fiske 1959). In this vein, I view asset pricing and implied cost of capital tests as complementary.

APPENDIX

DECOMPOSITION OF EXPECTED SHARE PRICE APPRECIATION

Noting that Value Line target prices are calculated from forecasts of future earnings multiplied by forecasted P/E ratios, *TP/P* can be decomposed as follows:

$$\begin{aligned}
\frac{TP}{P} &= (FEPS * FPE) \\
&= (AEPS + FE^{EPS}) * (APE + FE^{PE}) \\
&= (AEPS * APE) + (FE^{EPS} * FPE) + (AEPS * FE^{PE}) \\
&= (AEPS * APE) + (FE^{EPS} * FPE) + ((FEPS - FE^{EPS}) * FE^{PE}) \\
&= (AEPS * APE) + (FE^{EPS} * FPE) + (FEPS * FE^{PE}) - (FE^{PE} * FE^{EPS}) \\
&= (AP) + (FE^{EPS} * FPE) + (FEPS * FE^{PE}) - (FE^{PE} * FE^{EPS})
\end{aligned}$$

See Table 4 for variable definitions.

To fix ideas, consider the following simple numerical example. Let forecasted target price (*TP*) equal $45 per share, current stock price equal $20 per share (*P*), forecasted earnings per share equal $3.00, and forecasted P/E (*FCPE*) equal 15. Further, let the actual stock price turn out to be $35, with actual EPS equal to $2.50, implying an actual P/E (*APE*) of 14. The decomposition works out as:

$$\begin{aligned}
\frac{TP}{P} &= (FEPS * FPE) \\
\frac{45}{20} &= \left(\frac{3.00}{20} * 15\right) \\
&= (AEPS + FE^{EPS}) * (APE + FE^{PE}) \\
&= \left(\frac{2.50 + .50}{20}\right) * (14 + 1) \\
&= (AP) + (FE^{EPS} * FPE) + (FEPS * FE^{PE}) - (FE^{PE} * FE^{EPS}) \\
&= \left(\frac{2.50 * 14}{20}\right) + \left(\frac{.50 * 15}{20}\right) + \left(\frac{3.00 * 1}{20}\right) - \left(\frac{.50 * 1}{20}\right) \\
&= \left(\frac{35}{20}\right) + \left(\frac{7.50}{20}\right) + \left(\frac{3.00}{20}\right) - \left(\frac{.50}{20}\right)
\end{aligned}$$

The interaction term is needed since multiplying by *FCPE* in the second term and *FCEPS* in the third term (both of which are themselves too high) overstates the true effect of EPS and P/E forecast bias.

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Group-Based Compensation in Professional Service Firms: An Empirical Analysis of Medical Group Practices

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ABSTRACT: This study investigates factors influencing the use of group-based compensation in professional service partnerships and the relation between group-based compensation and performance. I use data on 11,971 physicians in 935 medical groups to expand the extant literature on group incentives by providing some of the first large-sample, empirical evidence on the role of task interdependence, income risk, mutual monitoring, and group size in a firm's choice of compensation contract. Consistent with agency theory, I find that group incentives are more prevalent in medical partnerships that practice highly task-interdependent specialties and those that face greater malpractice risk. Group-based incentives are also more common in relatively small groups in which homogeneity in training, experience, and gender facilitate mutual monitoring. Tests relating individual physician productivity to compensation method suggest that productive benefits induced by group incentives offset reductions in output associated with free-riding and effort devoted to monitoring.

Keywords: *group incentives; risk sharing; teams; partnerships; compensation.*

Data Availability: *Data are available from sources identified in the article.*

I. INTRODUCTION

This study identifies factors that influence the use of group incentives in labor-managed professional service firms and compares the performance effects of group incentives with those of individual incentives. Partnerships are the predominant form of organization in human-capital intensive professions such as accounting, law, consulting, and medicine. Professionals form partnerships to exploit productive interactions, share resources, and spread risk. In practice, partnerships employ a wide variety of compensation schemes or "sharing rules," with some partnerships rewarding members based solely on individual performance, while others base compensation on group performance and divide

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the group's net proceeds equally among partners (i.e., group-based or team-based compensation).

As compensation moves away from individual performance to group performance, the incentive to free ride on the effort of others increases. Traditional agency theory contends that firms weigh the benefits arising from group-based compensation, which include greater cooperation among group members and risk sharing, against losses in output from diminished incentives (e.g., Alchian and Demsetz 1972; Newhouse 1973; Holmstrom 1982). More recent work indicates that in certain settings, group incentives induce mutual monitoring and thereby lead to higher levels of effort (e.g., Kandel and Lazear 1992; Che and Yoo 2001; Huddart and Liang 2005). Taken together, this research suggests that firms will use group-based compensation under the following conditions: (1) work tasks are highly interdependent, (2) income risk is relatively high, (3) firm members can cost-effectively monitor each other, and (4) firm size is relatively small. An extensive theoretical literature analyzes compensation arrangements and free-riding in groups; yet, there is relatively little empirical research on the determinants and performance effects of group-based compensation (see Prendergast [1999] for a review).

I investigate the determinants and performance effects of group-based compensation using a database of 11,971 physicians in 935 medical group practices. This setting and data offer several advantages. First, physician compensation contracts tend to provide a relatively simple expression of the link between pay and performance. Second, compensation contracts vary by practice even though medical group practices operate in the same service sector (thereby controlling for confounding industry effects). Groups generally base compensation primarily on individual performance or primarily on group performance, with all members sharing equally in the group's net proceeds. Third, physician-level performance measures provide a unique opportunity to identify the effects of group incentives on *individual* performance. Prior research primarily examines group-level performance. Fourth, physician-level data measure factors that potentially influence both the use of group incentives and the effects of group incentives on individual performance. Variations in these factors afford for powerful tests of hypotheses suggested by theoretical models. Finally, rising expenditures for physician and clinical services, which totaled $447.6 billion in 2006, have drawn critical attention to the structure of physician incentives (Centers for Medicare & Medicaid Services 2008).

This study expands the extant literature on group incentives by providing some of the first large-sample, empirical evidence on the role of task interdependence, income risk, mutual monitoring, and group size in a firm's choice of compensation contract. Medical groups that practice highly task-interdependent specialties (e.g., emergency medicine) are significantly more likely to use group incentives than physicians who practice specialties with little opportunity for cooperation (e.g., psychiatry). The use of group incentives is positively associated with malpractice risk, suggesting that physicians use group-based compensation to insure themselves against income risk arising from malpractice lawsuits. Group incentives are also more prevalent in groups with relatively low mutual monitoring costs, as is the case with single-sex groups composed of members with similar training and experience. Finally, small- to medium-sized groups are more likely to use group incentives than larger groups.

This study also contributes to the literature by examining the individual performance effects of group incentives. Medical practices using group incentives exhibit significantly less variation in productivity than those utilizing individual incentive plans. More importantly, in contrast to the findings of prior empirical research (Encinosa et al. 1997), physicians whose compensation is based upon group performance are not less productive than those whose compensation is based upon individual performance. Thus, in physician firms, the productive benefits induced by group incentives offset reductions in output associated with free-riding and effort devoted to mutual monitoring. These findings are relevant for other labor-managed professional firms that share key organizational features with medical practices as well as corporations that increasingly require employees to work in teams.

Section II develops the research hypotheses. Section III discusses the sample and measures. Empirical tests are reported in Section IV. Results are discussed in Section V and conclusions are presented in Section VI.

II. BACKGROUND ON PHYSICIAN GROUP PRACTICES

The majority of medical doctors work in group practices owned by the doctors themselves. Non-owner doctors in physician-owned practices are typically new hires who will be granted an ownership interest after a few years of service (Lee 1990). Medical group practices range in size from three physicians to upward of 500, although most contain between five and 50 members (American Medical Association [AMA] 1999). I use the terms "partnership" and "group" interchangeably to refer to the provision of health care services by three or more physicians who are formally organized as a legal entity governed by physicians and who share business, clinical, and administrative facilities (AMA 1999, 1).

Physician-owned medical group practices are normally organized as limited liability corporations (LLC), limited liability partnerships (LLP), professional associations (PA), or partnerships.[1] The health care literature attributes the predominance of group practices in the medical profession to the ability to exploit economies of scale (Pauly 1996), smooth work schedules, and internalize referrals (Gaynor and Gertler 1995). In addition, groups have the potential to structure and coordinate care so as to achieve higher quality, greater patient satisfaction (Pauly 1996), and reputational economies of scale (Getzen 1984). Outside of the health care literature, Levin and Tadelis (2005) show that partnerships assure clients of higher levels of quality when quality is difficult to observe.

Although most physicians belong to group practices, the majority of physician outpatient services are provided in one-to-one interactions between a physician and a patient (Pauly 1996, 26). Differences in work habits, work pace, specialty, interpersonal skills, and other factors produce intra-group variations in the quantity, perceived quality, and cost of care provided, resulting in different contributions to the group's net earnings. Physician groups distribute earnings based upon individual performance or group performance. In the data set used for this study, 84 percent of practices use individual plans and 16 percent use group plans. Under individual performance plans, each physician receives some combination of salary and productivity-based pay determined by his/her individual performance. Physician firms normally use accounting metrics such as gross charges or adjusted charges to measure productivity. Under a group performance plan, each physician receives an equal share of the *entire* net proceeds of the group, regardless of individual output and effort. I refer to such incentive plans as equal-sharing, team-based compensation, and group-based compensation throughout the article.

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[1] In all four organizational forms, the net proceeds of the entity pass through to its owners who are taxed at the individual level. These organizational forms also offer limited liability, which prevents a group member from being legally responsible for the acts of another group member. Practices that are not owned by physicians are typically owned by hospitals and foundations. Such practices are excluded from the analysis because these physicians are employees of the entity that owns the practice.

III. HYPOTHESES

Determinants of Group-Based Compensation

In theory, firms choose group incentives if such incentives induce cooperative and insurance benefits that exceed the sum of losses in efficiency from free-riding and the costs associated with monitoring group members. In medicine, cooperative gains include higher levels of output and quality. However, under group incentive plans, physicians can free ride on their colleagues by seeing fewer patients or performing fewer procedures than their colleagues. To prevent free-riding, physicians can, at some cost, monitor their colleagues. For example, physicians can collect and review common productivity measures (e.g., charges, collections, or patient visits), discuss treatment approaches, or simply note the hours their colleagues maintain. To empirically test agency-based models of group incentives, I examine four factors that are predicted to influence the benefits and costs associated with team incentives: (1) task interdependence, (2) income risk, (3) the ability of group members to monitor each other, and (4) group size.

Task Interdependence

Groups are typically formed to take advantage of efficiency gains that accrue from cooperation. The management literature uses the term "task interdependence" to characterize the extent to which group members rely upon each other to perform their jobs (Wageman 1995; Wageman and Baker 1997; Van de Ven et al. 1976). The more task-interdependent a job is, the greater the gains from cooperation, coordination, and communication. When tasks are highly interdependent, it is difficult, if not infeasible, to directly link output to individual members' efforts and actions.

In medicine, the highest levels of task interdependence occur in inpatient settings (Pauly 1996). A team of physicians works together to perform a complex surgery or administer urgent care to a patient in an emergency room. Physicians in the hospital-based specialties of radiology and pathology coordinate with each other to process cases (e.g., diagnostic images or specimens) that come through their respective departments. Compared with inpatient settings, health care delivery in outpatient settings is much less task-interdependent. Yet, even in outpatient settings, intra-group coordination has the potential to improve collective output and quality, which in turn improve the group's reputation and increase the value of the partnership. For example, when a physician is unavailable, another partner often sees his/her patients so that they can be treated in a timely manner. Physicians often consult with each other on cases to determine the best treatment plan. Doctors also typically share "call duty" or the responsibility to care for any of the practice's patients on an urgent, as-needed basis, substantially enhancing the value of physicians' leisure time (Casalino et al. 2003).

When tasks are interdependent, agency models suggest that professionals can use group incentives to encourage cooperation and thereby improve output. Holmstrom and Milgrom (1991) and Itoh (1993) demonstrate that when it is Pareto-efficient for agents to coordinate their efforts, it is optimal for principals to use simple group incentive plans to encourage this activity. Arya et al. (1997) and Che and Yoo (2001) find that in team settings, principals should base compensation on group performance to induce members to monitor each other and punish free riders. Wageman and Baker (1997) show that when tasks are highly interdependent, group-based rewards are associated with increased output.

Empirical studies provide limited evidence on the performance effects associated with different combinations of task interdependence and reward system design. In a field study of 152 groups of service technicians, Wageman (1995) finds that highly task-interdependent groups perform better when rewards are based upon group performance rather than individual performance. Using an experimental study, Wageman and Baker (1997) report that cooperation leads to superior performance when interdependent tasks are accompanied by group rewards. Although results of both studies are consistent with theory, neither study examines the role of task interdependence in contract design. Two related accounting studies examine the role of organizational interdependencies in the design of compensation contracts for business unit managers in hierarchical corporations (see Bushman et al. [1995] and Keating [1997]). Interdependencies among multiple business units within the same firm reflect the extent to which a business unit manager's actions affect the performance of other business units. Empirical results suggest that the use of aggregate accounting performance measures (e.g., firm-wide operating income) relative to more direct performance measures (e.g., divisional operating income) is increasing in the level of organizational interdependencies.

Taken together, theoretical and limited empirical research suggest the follow hypothesis:

> **H1a:** The probability that a professional partnership bases compensation on group performance is increasing in the degree of task interdependence.

Income Risk Arising from a Malpractice Lawsuit

Physicians, like many professionals, invest heavily in their own human capital. Equal-sharing arrangements enable risk-averse professionals to ensure themselves against idiosyncratic shocks that impair the value of their human capital (Gaynor and Gertler 1995; Lang and Gordon 1995). Malpractice lawsuits are one example of a shock that can significantly reduce a professional's earning capability.[2] Medical malpractice liability exposes physicians to the risk that a patient experiences a bad outcome, as well as the uncertainty of the litigation process (Evans et al. 2006, 1046). While virtually all physicians are fully ensured against the majority of direct lawsuit expenses, including damages and legal fees, the indirect costs of a lawsuit can be substantial (Kessler and McClellan 1996). Lawsuits require a physician to spend time preparing a defense, reputation effects stemming from litigation reduce future revenues, and the likelihood of increased malpractice insurance premiums.

I focus on malpractice risk as a source of income risk because the legal environment significantly influences physician behavior. Prior research has shown that physicians who bear greater legal liability (due to state tort laws and court systems) seek to reduce overall financial risk by practicing "defensive medicine" and accepting less cost risk (through capitation) in their contracts with Managed Care Organizations (Kessler and McClellan 1996, 2000; Evans et al. 2006).[3] Physicians can also reduce financial risk by entering into equal-sharing arrangements with other physicians. If a lawsuit reduces the amount of revenues that one physician generates, this loss will be spread among all partners when compensation is based on group performance. The risk-sharing feature of group-based compensation plans leads to the following hypothesis:

> **H1b:** Professional partnerships located in geographic areas with higher malpractice risk will be more likely to compensate members based upon group performance than those practicing in areas with lower malpractice risk.

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[2] For physicians, other shocks include difficulty in fee collection, variation in insurance coverage and claim denial, no-shows, shifts in demand for particular specialties, and simply luck (Gaynor and Gertler 1995, 593).

[3] Kessler and McClellan (1996, 254) define "defensive medicine" as the provision of "precautionary medical treatments that have minimal expected medical benefit out of fear of legal liability."

Mutual Monitoring

Theoretical research has shown that when it is advantageous to use team-based compensation, mutual monitoring and peer pressure can mitigate free-riding (e.g., Narayanan 1995; Miller 1997; Che and Yoo 2001; Huddart and Liang 2005). If public signals of partners' individual efforts exist, group members can use these signals as grounds for imposing explicit or implicit sanctions upon fellow group members (Hansmann 1996). Most theoretical models rely on explicit contractual sanctions such as monetary penalties or the threat of expulsion.[4] However, multi-period models demonstrate that peer pressure can create implicit sanctions, including guilt, shame, and envy, which deter free-riding (e.g., Milgrom 1988; Lazear 1989; Arya et al. 1997).

Whether explicit or implicit, the effectiveness of mutual monitoring and peer pressure is likely to increase in the homogeneity of team members. Quite simply, it is easier for a group member to monitor someone who is like himself in training, job responsibility, and work patterns than it is to monitor someone who differs on these factors (Kandel and Lazear 1992; Encinosa et al. 1997). Accordingly, homogeneity reduces monitoring costs and the incentive to shirk monitoring responsibilities. Homogeneity also increases the power of peer pressure. Peer pressure relies on partners feeling empathy for each other, and empathy is likely to be stronger among partners who are similar. The assumption that mutual monitoring and peer pressure are more effective in homogeneous groups leads to Hypothesis 1c.

> **H1c:** The probability that a professional partnership bases compensation on group performance is decreasing in the diversity of its members.

Group Size

Theoretical models provide competing perspectives regarding the effect of group size on sharing. Pay-performance intensity decreases as group size increases; hence, the incentive for group members to supply effort and to monitor others decreases with size (e.g., Alchian and Demsetz 1972; Holmstrom 1982; Huddart and Liang 2004). Conversely, the insurance benefits inherent in equal sharing plans predict a positive association between size and sharing. As group size increases, income risk is spread over more partners, so that the income variance for any given sharing rule is smaller in larger groups (Lang and Gordon 1995; Encinosa et al. 1997). Accordingly, higher levels of income insurance in larger groups suggest that, *ceteris paribus*, sharing will be greater in larger groups.

Direct empirical evidence on the relation between group size and equal-sharing in partnerships is limited and mixed. Lang and Gordon (1995) find that profit-sharing in 329 law practices increases significantly as firm size increases from fewer than 21 lawyers to 21–30 lawyers, but profit-sharing does not increase as size increases beyond 30 members. In a study of 583 medical groups, Encinosa et al. (1997) find that use of equal-sharing decreases as group size increases. Institutional differences potentially explain the contrasting results for lawyers and physicians.[5] Also, neither study controls for several factors that are correlated with both group size and compensation plan, namely experience, intra-group diversity, income risk, and geographic area. Finally, Encinosa et al. (1997) use survey data from 1978, which may not be relevant to the current organization of medical practices.[6] Accordingly, this issue warrants further empirical investigation. Still, prior evidence of free-riding in physician firms suggests the following hypothesis:

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[4] In one-period models with budget-balancing, explicit sanctions require that a partner or group of partners outside the group being evaluated serve as the "sink" to administer punishments and rewards (Miller 1997). Huddart and Liang (2005) preserve budget-balancing with explicit sanctions by distributing the risk associated with the payment schedule across the other partners of the firm. Narayanan (1995) and Che and Yoo (2001) use multi-period games to relax the budget-balancing constraint and enable partnerships to correlate reward with effort through threat of expulsion or non-cooperation in future periods.

H1d: The probability that a professional partnership compensates members based upon group performance is decreasing in group size.

Effects of Team-Based Compensation on Performance

Firms choose compensation systems to attract desirable members and encourage them to take appropriate actions. The following sections consider evidence on the relation between group incentives and performance.

Intra-Group Variation in Productivity

Theoretical research suggests that group incentives work best with individuals of similar ability and work ethic. Partnerships composed of high-ability professionals are reluctant to offer partnership status to low-ability professionals to avoid subsidizing less-able partners (Farrell and Scotchmer 1988). Similarly, high-ability professionals select partnerships with members whose abilities match their own. As a result, professionals sort into partnerships of roughly equal ability. Aside from the selection effects created by team-based compensation plans, Kandel and Lazear's (1992) model suggests that peer pressure creates incentives that further minimize deviations from group productivity norms.

Empirical evidence on the performance effects of group incentives is primarily limited to Weiss' (1987) case study of 752 newly hired manufacturing workers. He finds that output variability decreases as workers progress from a piece-rate plan in the first month of employment to a group incentive plan in the fourth month. Analysis of worker "quit rates" suggests that group-based compensation drives out the low-ability and high-ability workers, while retaining medium-ability workers. Although Weiss (1987) provides much-needed empirical evidence on the effects of group incentives, it is not clear whether his results hold for professionals. The selection and incentive effects of group-based compensation suggest the following hypothesis:

H2: Intra-firm variation in productivity will be lower in professional partnerships that base compensation on group performance than it is in those that base compensation on individual performance.

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[5] To the extent that demand for physician services is stronger and less variable than that for law services, physicians will have less need for income insurance than lawyers (Cooper et al. 2002). Leibowitz and Tollison (1980) posit that brand-name capital and intra-firm referrals make larger law firms economically efficient. Also, legal work is generally more task interdependent than medicine.

[6] During the 20 years that have elapsed between Encinosa et al.'s (1997) survey and the 1998 survey used in this study, fundamental changes in the organization of medical practices and the level of income risk borne by physicians have significantly affected contract design. In 1978 most physicians were U.S.-born males practicing alone or in small, loosely structured clusters (Mauney 2002). By 1998, a more diverse set of physicians practiced in a wider variety of settings and organizations. Group size increased and a greater percentage of physicians began practicing in groups. Professional corporations offering limited liability replaced partnerships and sole proprietorships as the dominant organizational form. Health systems and managed care organizations began hiring physicians on a salary basis. Income risk increased from 1978 to 1998 due to increases in malpractice risk, greater limitations on reimbursements from Medicare and other third-party payers, and greater use of capitation contracts (Mauney 2002; Leone 2002).

Individual Productivity

Agency theory contends that, *ceteris paribus*, productivity per worker will be lower under group incentive plans than under individual incentive arrangements because group incentive plans weaken the link between individual performance and compensation (e.g., Alchian and Demsetz 1972; Holmstrom 1982; Holmstrom and Milgrom 1991). However, as previously noted, recent theoretical studies suggest that in certain settings, productivity will be no different, or even higher, under group incentives plans (e.g., Kandel and Lazear 1992; Arya et al. 1997; Che and Yoo 2001; Huddart and Liang 2005).

Despite the large theoretical literature on group incentives plans, direct empirical evidence is sparse (Prendergast 1999). Three particularly relevant empirical studies use data from a 1978 survey of 6,353 physicians in 957 medical group practices (Gaynor and Pauly 1990; Gaynor and Gertler 1995; Encinosa et al. 1997).[7] Gaynor and Pauly (1990) and Gaynor and Gertler (1995) find that productivity is reduced when individual incentives are weakened either by introducing a fixed salary component to individual compensation or by increasing the degree of revenue-sharing among group members. Consistent with these claims, Encinosa et al. (1997) find that productivity in equal-sharing groups containing more than five members is significantly lower than productivity in groups using individual plans.

Limitations in the 1978 data used in these studies and significant changes in the health care environment (see footnote 6) suggest that additional insight can be gained by revisiting equal-sharing arrangements with a larger, more comprehensive, and more recent data set. In particular, I use the individual physician as the unit of analysis instead of the group. Unlike prior studies, I include controls for factors that are correlated with both productivity and contract design (clinical hours, subspecialty, and malpractice risk). Additionally, my productivity measures (charges and income) better reflect service complexity than the 1978 survey measure (office visits per hour).

Limited empirical evidence on physicians suggests that weakening the pay-performance link lowers output and thus provides the basis for Hypothesis 3:

> **H3:** Individual productivity will be lower for professional partnerships that base compensation on group performance than those that base compensation on individual performance.

IV. RESEARCH DESIGN

Sample

The data for this study come from a nationwide survey developed and conducted by the Medical Group Management Association (MGMA 1999a). Surveys were mailed to 5,193 MGMA member practices in February of 1999. Responses were received from 1,772 practices. MGMA staff eliminated 163 surveys due to incomplete data reporting, duplication, or late submission, yielding an adjusted response rate of 30.98 percent. Consistent with the AMA's definition of group practice, I delete groups with fewer than three members and those not owned by physicians (AMA 1999, 1). I also delete not-for-profit and medical school faculty practices, which have unique objectives. The final sample consists of 11,971 physicians in 935 practices.[8]

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[7] Gaynor and Pauly (1990) and Encinosa et al. (1997) conduct analyses at the group-level using 905 and 488 groups, respectively. Gaynor and Gertler (1995) analyze a subset of 1,230 individual physicians.

Sample data vary by group and by individual physician. The unit of analysis for tests of H1a–H1d and H2 is the group; physician-level variables, such as experience, are aggregated to the group level for these tests. The unit of analysis for tests of H3 is the physician. Table 1 reports variable definitions, and Tables 2 and 3 report descriptive statistics for group-level variables and physician-level variables, respectively. Uppercase and lowercase letters denote group-level and physician-level variables, respectively.

Measurement of Compensation Method

Compensation method is determined from a survey question that asks respondents to indicate the basis of compensation for each practice member, as follows:

Please select the compensation method that most closely resembles how each provider's compensation is computed.

1. **100% Equal Shares:** All providers in the practice are paid equal shares based upon overall practice performance.
2. **100% Productivity:** The provider's compensation is based wholly on individual productivity measures (gross charges, encounters, RVUs, etc.).
3. **50% to 99% Productivity:** Between 50 percent and 99 percent is based upon individual physician productivity measures. The remainder of the compensation is a set amount. If a provider is compensated 50 percent from productivity and 50 percent is guaranteed salary, select option 2.
4. **50% to 99% Guaranteed Salary:** Between 50 percent and 99 percent of the provider's compensation is paid by a base or guaranteed amount, the balance of compensation is determined by other factors (e.g., productivity, bonus, capitation). If 50 percent of the provider's compensation is from a guarantee salary and 50 percent is from productivity measures, select option 2, 50% to 99% Productivity.
5. **100% Straight Salary:** The compensation for the provider is 100 percent attributable to a fixed salary.

The first category, "100% Equal Shares," identifies groups that divide 100 percent of net proceeds equally among all members. The indicator variable for group-based compensation, *GROUP COMP*, equals 1 for these practices. One-hundred fifty practices (16 percent) use group-based compensation. The remaining 785 practices (84 percent), which fall

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[8] The AMA (1999) survey provides the most comprehensive data set on physician group practices available for this time period. MGMA sample practices are larger than those in the AMA sample, but comparable with respect to specialty composition. Median MGMA group size is similar to AMA group size (7 versus 6 physicians, respectively), but average group size for the MGMA sample is larger (12.8 versus 10.0 physicians, respectively). The proportion of single-specialty groups in the MGMA sample (77 percent) is highly similar to that of AMA groups (78 percent). The MGMA sample is also very comparable to the AMA sample with respect to the number of physicians in family practice (13 percent for both) and the internal medicine specialties (34 percent and 32 percent, respectively). However, the MGMA sample has relatively more physicians that practice a surgical specialty (34 percent and 25 percent, respectively).

TABLE 1
Variable Definitions[a]

Variables		Definitions
Dependent Variables		
GROUP COMP	=	indicator variable that equals 1 if a predominant number of group members share equally in the net proceeds of the group, and 0 otherwise.
group comp	=	indicator variable that equals 1 if physician receives an equal share of the group's net proceeds.
100% productivity	=	indicator variable that equals 1 if physician's entire compensation is based upon individual productivity, and 0 otherwise.
50%–99% productivity	=	indicator variable that equals 1 if 50–99 percent of a physician's compensation is based upon individual productivity, and 0 otherwise.
50%–99% salary	=	indicator variable that equals 1 if 50–99 percent of a physician's compensation is guaranteed salary and the remainder is based on individual productivity.
100% salary	=	indicator variable that equals 1 if 100 percent of a physician compensation physician is guaranteed salary.
AVG CHARGES	=	average of gross charges (full retail prices billed) for the group.
STD CHARGES	=	intra-group standard deviation of gross charges.
CHARGE VARIATION	=	within group standard deviation of gross charges (full retail prices billed) divided by average gross charges for the group.
charges	=	full retail prices of services billed by an individual physician.
income	=	total compensation, including salary, bonuses, incentive payments, research stipends, and profit distributions for an individual physician.
Independent Variables		
INTERDEPEND	=	task interdependence.
RISK	=	Medicare malpractice geographic practice cost index.
SPECIALTY DIVERSITY	=	number of different specialties in group divided by group size.
FTE DIVERSITY	=	standard deviation of the proportion of full-time-equivalent units each group member devotes to clinical activities.
GENDER DIVERSITY	=	proportion of group members belonging to the non-dominant gender.
3–6, 7–10, 11–20, 21–31, and *> = 31 PHYSICIANS*	=	indicator variables set equal to 1 if number of physicians in group falls within specified size range.
SIZE	=	number of physicians in group.
LN SIZE	=	natural log of the number of physician in the group.
CAPITATION	=	Ln(1 + percentage of group revenues derived from capitation contracts).
MCO PENETRATION	=	managed care penetration by county.
COMPETITION	=	physicians per 100,000,000 in population in county.
SUBURB, RURAL, and *URBAN*	=	indicator variables that equal 1 if the medical group is located in a rural, suburban, or urban area, respectively.
% < = 3 YEARS	=	percentage of physicians in group with fewer than three years of experience.

(*continued on next page*)

TABLE 1 (continued)

Variables		Definitions
NORTHEAST, NORTH ATLANTIC, MIDATLANTIC, SOUTHEAST, ROCKY MOUNTAIN, NORTHWEST, SOUTHWEST, EASTERN MIDWEST, UPPER MIDWEST, LOWER MIDWEST, N. CALIFORNIA, SO. CALIFORNIA, etc.	=	indicator variables that equal 1 if group is located in specified region; *UPPER MIDWEST* is the omitted region in tests of the hypotheses.
clinical FTE	=	proportion of full-time equivalent units (FTEs) a physician devotes to clinical activities.
years	=	years experience practicing medicine.
< = *2 years, 3–5 years,* > *25 years*	=	indicator variables that equal 1 if physician has been practicing medicine for 2 or fewer years, 3 to 5 years, or more than 25 years, respectively, and 0 otherwise.
male	=	indicator variable that equals 1 if physician is male, and 0 otherwise.
subspecialty	=	indicator variable that equals 1 if physician has a subspecialty, and 0 otherwise.

[a] Group-Level variables are in *UPPERCASE* letters and Physician-Level variables are in *lowercase* letters.

into one of the last four categories, base compensation on individual performance (*GROUP COMP* = 0).[9,10]

Wording of the compensation question raises some ambiguity regarding classification if compensation plans incorporate both individual and group incentives. For example, suppose 60 percent of compensation is based on individual productivity, with the remainder equally split among all partners. As respondents are instructed to choose the method that "most closely resembles how each provider's compensation is computed," respondents likely will classify such a plan as "50% to 99% Productivity." Thus, providers classified as receiving "50% to 99% Productivity" may have a substantial portion (up to 49 percent) of their compensation based upon group performance. Similarly, sharing may also be used with providers classified as receiving "50% to 99% Guaranteed Salary." To address this concern, I estimate the model's predicting compensation method both with and without observations with compensation plans classified as "50% to 99% Productivity" and "50% to 99% Guaranteed Salary." To the extent that plans incorporating sharing are included in these categories, empirical tests using the full sample will be biased against the hypothesized predictions.

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[9] Ittner et al. (2007) identify factors associated with the choice of *individual* compensation methods (100% Productivity, 50% to 99% Productivity, 50% to 99% Guaranteed Salary, 100% Straight Salary), but do not consider equal-sharing arrangements. Their empirical tests of survey response data indicate that compensation plans were correctly classified (Ittner et al. 2007, 309–310). The survey also allowed respondents to indicate "other" if none of the five responses listed was appropriate; less than 1 percent of physicians were placed in this category.

[10] Medical groups often use different compensation schemes with different members (Lee 1990). On average, sample groups used the same compensation method with 90 percent of their members. The compensation method used with the greatest number of members determines the compensation method assigned to the group.

TABLE 2

Descriptive Statistics for Variables Predicting Use of Group-Based Compensation and Variation in Productivity for 935 Physician Groups, Partitioned by Compensation Method

	All Groups (n = 935)			*GROUP COMP* = 1 (n = 150)			*GROUP COMP* = 0 (n = 785)			Difference between Partitions	
	Mean	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**	**t^a**	**M-W^b**
Task Interdependence											
INTERDEPEND	2.66	2.57	0.47	2.87	2.79	0.53	2.62	2.53	0.45	(0.000)	(0.000)
Income Risk											
RISK	0.83	0.79	0.34	0.85	0.79	0.40	0.83	0.80	0.32	(0.445)	(0.920)
Intra-Group Diversity											
SPECIALTY DIVERSITY	0.29	0.25	0.17	0.22	0.20	0.13	0.31	0.25	0.18	(0.000)	(0.000)
FTE DIVERSITY	0.07	0.00	0.10	0.06	0.00	0.10	0.07	0.00	0.10	(0.299)	(0.251)
EXPERIENCE DIVERSITY	7.86	7.85	3.01	7.26	7.33	2.91	7.98	7.92	3.02	(0.007)	(0.006)
GENDER DIVERSITY	0.10	0.00	0.13	0.81	0.00	0.12	0.11	0.05	0.13	(0.001)	(0.008)
Group Size											
SIZE	12.80	7.00	21.02	9.39	8.00	7.10	13.46	7.00	22.68	(0.030)	(0.819)
LN SIZE	2.11	1.95	0.79	2.03	2.08	0.62	2.13	1.95	0.82	(0.184)	(0.819)
3–6 PHYSICIANS	0.45			0.43			0.45			(0.584)	
7–10 PHYSICIANS	0.25			0.31			0.23			(0.060)	
11–20 PHYSICIANS	0.19			0.18			0.19			(0.834)	
21–31 PHYSICIANS	0.04			0.07			0.04			(0.044)	
> = 31 PHYSICIANS	0.08			0.01			0.09			(0.001)	

(*continued on next page*)

TABLE 2 (continued)

	All Groups (n = 935)			*GROUP COMP* = 1 (n = 150)			*GROUP COMP* = 0 (n = 785)			Difference between Partitions	
	Mean	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**	**Mean**	**Median**	**Std. Dev.**	**t[a]**	**M-W[b]**
Physician Experience											
% < = 3 YEARS	11.60	8.33	13.79	10.01	4.00	13.66	11.90	8.33	13.81	(0.124)	(0.084)
Market Conditions											
CAPITATION[c]	0.77	0.00	1.23	0.58	0.00	1.11	0.80	0.00	1.25	(0.050)	(0.046)
MCO PENETRATION	0.26	0.26	0.16	0.29	0.27	0.14	0.26	0.26	0.17	(0.049)	(0.042)
COMPETITION	0.29	0.25	0.18	0.28	0.25	0.18	0.31	0.28	0.18	(0.036)	(0.009)
SUBURB	0.49			0.45			0.50			(0.226)	
RURAL	0.08			0.01			0.09			(0.001)	
Intra-Group Variation in Productivity											
CHARGE VARIATION[c]	0.34	0.31	0.20	0.25	0.23	0.16	0.35	0.32	0.20	(0.000)	(0.000)

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[a] Significance of independent samples t-test for equality of means for groups using group-based compensation and those using individual compensation.

[b] Significance of Mann-Whitney U-statistic testing whether samples of groups are drawn from same population.

[c] n for *CAPITATION* is 924 (147 group, 777 individual); n for *CHARGE VARIATION* is 865 (131 group; 734 individual).

Variables are defined in Table 1.

Physician-level tests of H3 incorporate all five compensation categories (100% Equal Shares, 100% Productivity, 50% to 90% Productivity, 50% to 99% Guaranteed Salary, 100% Straight Salary); and, the corresponding physician-level indicator variables are: *group comp, 100% productivity, 50%–99% productivity, 50%–99% salary,* and *100% salary.* Table 3 (Panel A) contains the distribution of compensation methods across the 11,234 physicians in the sample.

Measurement of the Determinants of Group Incentives

Task Interdependence

A physician's specialty largely determines task interdependence. The extant literature suggests that task interdependence is influenced by three factors: the importance of care continuity in the physician-patient relationship, the amount of *individual* influence a physician exerts over his patient panel, and the extent to which a physician controls the timing and amount of work performed (Schwartz et al. 1989; Nutting et al. 2003; Saultz 2003; Taylor 2003). Continuity of care refers to the longitudinal interpersonal physician-patient relationship that transcends multiple illness episodes and includes responsibility for preventative care and care coordination (Saultz 2003). When continuity is important to care delivery, one physician cannot be easily substituted for another without compromising patient satisfaction and quality; thus, task interdependence is low. Continuity of care is most important in primary care specialties (e.g., family practice) and least important in hospital-based specialties (e.g., radiology), in which physicians typically treat patients on a nonrecurring basis (Nutting et al. 2003; Saultz 2003). The more influence any one individual physician can exert on his/her patient panel, the less dependent that physician is on the assistance of his colleagues to be successful, and the lower the degree of task interdependence (Atlantic Information Services, Inc. 2002). Physicians often distinguish their services from those of their colleagues by building strong reputations that lead to referrals. Some physicians attract patients by marketing their services and offering extended hours. Ophthalmologists, for example, actively advertise vision correction procedures to attract patients. Conversely, an emergency room physician treats whoever comes to the emergency room during his/her shift.

A physician's control over the hours worked each week varies dramatically by specialty (Taylor 2003). Schwartz et al. (1990) refer to specialties that allow the physician to control the number of hours they work as "controllable lifestyle" specialties. These include dermatology, neurology, ophthalmology, and psychiatry. Conversely, "non-controllable lifestyle" specialties, which include most surgical specialties, require physicians to work long and unpredictable hours. Physicians in non-controllable lifestyle specialties can make their work schedules more manageable by coordinating patient care and covering for one another (i.e., sharing "call"). Thus, "non-controllable lifestyle" specialties are characterized by high levels of task interdependence because group members rely heavily upon each other to perform their jobs.

The Appendix summarizes results of a survey that asked physicians to evaluate different specialties with respect to continuity, influence on patient panel, and control over work schedule. The results are highly consistent with the practitioner literature on specialties. I measure task interdependence, denoted *interdepend,* with an average of the physician ratings for continuity, panel influence, and schedule control. Larger values for *interdepend* indicate higher levels of task interdependence. A group-wide measure of task interdependence, denoted *INTERDEPEND,* is calculated using the average of the individual physician-level task interdependence scores.

TABLE 3
Physician-Level Descriptive Statistics for Compensation Method, Productivity, Physician Characteristics, and Specialty

Panel A: Distribution of Compensation Methods

	n	Percentage
group comp	1,094	9.7
Individual Compensation Plans		
100% productivity	3,961	35.3
50–99% productivity	2,705	24.1
50–99% salary	2,144	19.1
100% salary	1,330	11.8
Total	11,234	100.0

Panel B: Productivity Data and Individual Physician Characteristics, Partitioned by Compensation Method

	All Physicians n = 11,234			*group comp* = 1 n = 1,094			*group comp* = 0 n = 10,140			Tests for Equality of Partitions	
	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	Mean	Median	Std. Dev.	t[a]	M-W[b]
Productivity											
charges[c]	$899,359	$714,624	$745,465	$1,107,650	$923,000	$622,862	$876,888	$684,000	$754,106	(0.000)	(0.000)
income	$258,794	$213,921	$175,632	$345,389	$305,929	$174,821	$249,451	$204,832	$173,158	(0.000)	(0.000)
Physician Characteristics											
clinical FTE	0.959	1.00	0.14	0.970	1.00	0.10	0.958	1.00	0.14	(0.008)	(0.211)
years	13.78	12.00	8.91	14.81	14.00	7.52	13.67	12.00	9.04	(0.000)	(0.000)
< = 2 years	7.3%			1.8%			7.9%			(0.000)	
3–5 years	13.0%			8.4%			13.5%			(0.000)	
> 25 years	10.8%			8.1%			11.1%			(0.003)	
male	85.6%			93.4%			84.7%			(0.000)	
subspecialty	12.3%			11.2%			12.4%			(0.278)	

(*continued on next page*)

TABLE 3 (continued)

Panel C: Distribution of Physician Specialties

Specialty	Specialty Groupings[6]	Frequency	Percentage
Allergy	Other	116	1.03
Anesthesiology	Hospital-based	690	6.14
Cardiology	Internal medicine subspecialty	1,135	10.11
Dermatology	Other	125	1.11
Emergency	Hospital-based	139	1.24
Family practice	Primary care	1,415	12.60
Gastroenterology	Internal medicine subspecialty	326	2.90
Hematology/oncology	Internal medicine subspecialty	200	1.78
Internal medicine	Primary care	1,240	11.04
Nephrology	Internal medicine subspecialty	140	1.25
Neurology	Other	206	1.83
Nuclear medicine	Hospital-based	13	0.12
Obstetrics/gynecology	Primary care/surgical	717	6.38
Ophthalmology	Surgical	361	3.21
Orthopedic surgery	Surgical	1,278	11.38
Otolaryngology	Surgical	220	1.96
Pathology	Hospital-based	80	0.71
Pediatrics	Primary care	731	6.51
Physiatry	Other	74	0.66
Psychiatry	Other	65	0.58
Pulmonary medicine	Internal medicine subspecialty	197	1.75
Radiology	Hospital-based	416	3.70
General surgery	Surgical	471	4.19

(*continued on next page*)

TABLE 3 (continued)

Panel C: Distribution of Physician Specialties

Specialty	Specialty Groupings[6]	Frequency	Percentage
Cardiothoracic surgery	Surgical	218	1.94
Colon surgery	Surgical	14	0.12
Neurological surgery	Surgical	139	1.24
Plastic surgery	Surgical	55	0.49
Vascular surgery	Surgical	87	0.77
Urology	Surgical	366	3.26
Total		11,234	100.00

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[a] Significance of independent samples t-test for equality of means for physicians compensated based on group performance (*group comp* = 1) and those compensated based on individual performance (*group comp* = 0).

[b] Significance of Mann-Whitney U-statistic testing whether samples (*group comp* = 0 and *group comp* = 1) are drawn from same population.

[c] For *charges*, n = 10,074 (981 group; 9,093 individual).

Variables are defined in Table 1.

Malpractice Liability

Following Evans et al. (2006), I measure the risk of financial loss arising from malpractice lawsuits (*RISK*) with Medicare's 1999 Malpractice Geographic Practice Cost Index (MGPCI). The MGPCI reflects geographic variations in malpractice costs faced by physicians. The Health Care Financing Administration (HCFA) calculates the MGPCI from insurer premium and claim data (Task Order 0038, HCFA 2000).

Intra-Group Diversity

Physicians within the same group may differ on several factors that influence monitoring ability, including medical specialization, clinical responsibility, experience, and gender. Physicians in the MGMA sample practice in one of 29 different specialties (Panel C, Table 3). I measure intra-group diversity among specialties by dividing the number of different specialties in the group by the number of group members (*SPECIALTY DIVERSITY*). When this variable equals its maximum of 1, every member of the group is in a different specialty.[11] Physicians in the same group sometimes devote different amounts of time to clinical activities. Administrative responsibility, teaching, and continuing professional education reduce the time available to treat patients. I measure intra-group diversity in clinical work (*FTE DIVERSITY*) using the within-group standard deviation of the amount of clinical full-time-equivalent units (FTEs) reported for each group member (*clinical FTE*). Intra-group diversity in physician experience (*EXPERIENCE DIVERSITY*) is measured using the within-group standard deviation of the number of years each group member has been practicing medicine (*experience*). I measure the relative number of males and females in a group (*GENDER DIVERSITY*) by subtracting the percentage of a group's physicians that are of the same gender as the majority of physicians in the group from 1.[12] All four measures of intra-group diversity are expected to be negatively associated with the use of group incentives.

Group Size

To accommodate potential changes in the direction of the relation between size and the use of team-based compensation, group I measure size with five indicator variables (Kandel and Lazear 1992). The following variables, *7–10 PHYSICIANS, 11–15 PHYSICIANS, 16–20 PHYSICIANS, 21–30 PHYSICIANS*, and *>=31 PHYSICIANS*, equal 1 if the number of physicians in the group falls within the respective size category,, and 0 otherwise. The omitted category represents practices with fewer than seven physicians. Group size is also measured with the natural log of the number of physicians in a group (*LN SIZE*).

Control Variables

I control for a group's propensity to use salary with new physicians by including a variable that measures the percentage of a group's physicians that have been practicing medicine fewer than four years (<= *3 YEARS*).[13] I also control for the following market-related factors that potentially affect physician incentives (Hurley et al. 1996; Leone, 2002; Evans et al. 2006):

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[11] Specialty diversity is also computed using the sum of the squares of the proportion of group members in each specialty and the log of the number of different specialties in the group. These measures yield materially similar results in tests of the hypotheses.

[12] I include a measure of the relative mix of female and male physicians because research suggests that female physicians differ systematically from their male counterparts in their practice patterns, relationships with patients, approach toward care delivery, and attitude toward compensation (Kletke et al. 1990; Levinson and Lurie 2004; Rizzo and Zeckhauser 2007). These factors are likely to affect monitoring efficiency. *GENDER DIVERSITY* is highly and positively correlated with the percentage of female group members. Studies from the economics and psychology literatures find that women are more risk-averse than men (e.g., Powell and Ansic 1997; Sunden and Surette 1998; Bajtelsmit et al. 1999; Dwyer et al. 2002). If this is the case, then women should prefer group incentives, and the correlation between *GENDER DIVERSITY* and the percentage of females in the group will bias the results against H1c.

CAPITATION = ln(1 + percentage of revenues derived from capitation contracts) measures capitation. A log transformation is used because the capitation percentage is skewed to the right;
MCO PENETRATION = total Health Maintenance Organization (HMO) enrollment per 10,000 in population;
COMPETITION = physicians per 100,000 in population;
URBAN, SUBURBAN, RURAL = indicator variables specifying whether the practice is located in an urban, suburban, or rural metropolitan area. *URBAN* is the omitted variable; and
$REGION_i$ = indicator variables representing 11 different regions of the country. The indicator variable for the upper Midwest region is the omitted variable.

Variables measuring managed care penetration and physician competition come from the Area Resource File. All other market variables come from the MGMA survey.

Hypotheses 1a–1d, which address determinants of group-based compensation, are analyzed at the group level using the following cross-sectional logistic regression model.

$$\begin{aligned} \mathit{GROUP\ COMP} = {} & \alpha + \beta_1 \mathit{INTERDEPEND} + \beta_2 \mathit{RISK} \\ & + \beta_3 \mathit{SPECIALTY\ DIVERSITY} + \beta_4 \mathit{FTE\ DIVERSITY} \\ & + \beta_5 \mathit{EXPERIENCE\ DIVERSITY} + \beta_6 \mathit{GENDER\ DIVERSITY} \\ & + \beta_7 \mathit{7\text{–}10\ PHYSICIANS} + \beta_8 \mathit{11\text{–}15\ PHYSICIANS} \\ & + \beta_9 \mathit{16\text{–}20\ PHYSICIANS} + \beta_{10} \mathit{21\text{–}30\ PHYSICIANS} \\ & + \beta_{11} \mathit{>31\ PHYSICIANS} + \beta_{12} \mathit{\leq 3\ YEARS} \\ & + \beta_{13} \mathit{CAPITATION} + \beta_{14} \mathit{MCO\ PENETRATION} \\ & + \beta_{15} \mathit{COMPETITION} + \beta_{16} \mathit{SUBURBAN} + \beta_{17} \mathit{RURAL} \\ & + \Sigma_{i=18}^{27}\, \beta_i \mathit{REGION}_i. \end{aligned} \tag{1}$$

Physician Productivity

To test H2, I regress intra-group variation in productivity on the indicator variable for group-based compensation and a set of control variables. I measure intra-group variation in productivity (*CHARGE VARIATION*) by dividing the within-group standard deviation in gross charges by average charges. Gross charges are the full-dollar value of services provided at the practice's established undiscounted rates before reduction by Medicare fee restrictions, charitable adjustments, contractual adjustments, bad debts, and other adjustments. Gross charges capture the time and intensity of physician services. Control variables included in the OLS regression model (Equation (2)) that tests H2 are *INTERDEPEND* and the aforementioned measures of diversity, size, malpractice risk, and market conditions.

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[13] The omitted category potentially controls for partnership status because medical groups typically do not admit new physicians into the partnership until after three years. In reporting descriptive statistics, the MGMA assumes that physicians with three or more years of experience are partners, but the survey does specifically ask which physicians are partners.

$$\begin{aligned} \mathit{CHARGE\ VARIATION} = {} & \alpha + \beta_1 \mathit{GROUP\ COMP} + \beta_2 \mathit{INTERDEPEND} \\ & + \beta_3 \mathit{RISK} + \beta_4 \mathit{SPECIALTY\ DIVERSITY} \\ & + \beta_5 \mathit{FTE\ DIVERSITY} + \beta_6 \mathit{EXPERIENCE\ DIVERSITY} \\ & + \beta_7 \mathit{GENDER\ DIVERSITY} + \beta_8 \mathit{LN\ SIZE} \\ & + \beta_9 \mathit{CAPITATION} + \beta_{10} \mathit{MCO\ PENETRATION} \\ & + \beta_{11} \mathit{COMPETITION} + \beta_{12} \mathit{SUBURBAN} \\ & + \beta_{13} \mathit{RURAL} + \Sigma_{i=14}^{23}\, \beta_i \mathit{REGION}_i. \end{aligned} \tag{2}$$

The individual physician is the unit of analysis in tests of H3, which examines the performance effects of group-based incentives. I measure productivity with gross charges (*charges*) and total compensation per physician (*total pay*). Both variables are highly skewed to the right; therefore, I also include regressors for the natural logarithms of these variables (*ln charges* and *ln total pay*). Gross charges capture clinical productivity.[14] Total compensation includes salary, bonuses, incentive payments, research stipends, and profit distributions, reflecting effort in clinical and non-clinical tasks. Median charges and income for physicians with group incentive plans ($923,000 and $305,929, respectively) significantly exceed the corresponding amounts for physicians with individual incentive plans ($684,000 and $204,832, respectively) (Table 3 Panel B). The following variables control for the many factors outside the compensation contract that affect charges and income:

clinical FTE = the percentage of full-time-equivalent units (FTEs) that a physician devotes to clinical work. The median value for this variable is 1, as most physicians in the sample spend all of their time on clinical activities;

<= 2 years, *3–5 years*, and *>25 years* = indicator variables that equal 1 if a physician has been practicing in her chosen specialty for the corresponding time period. Prior research suggests that productivity improves with experience for most of a physician's work life, but is eventually counteracted by increasing age (Gaynor and Pauly 1990; Gaynor and Gertler 1995);

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[14] Gross charges do not include adjustments for Medicare fee restrictions, contractual adjustments, and other adjustments, which range from 20 percent to 50 percent of total group revenues (MGMA 1999b). Total adjustments vary widely among practices, and thus reflect inter-group differences in income risk. The data set does not provide information on adjustments at the physician level; however, net revenues (collections) are available at the group level for a subset of practices. To control for variation in income risk from adjustments, I included the natural log of net revenue as a dependent variable in robustness tests of H3. The results were highly consistent with the findings presented in the article. Inclusion of net revenue reduced sample size by approximately 40 percent; therefore, this control variable is not included in the final models.

male = indicator variable that equals 1 if the physician is male, and 0 otherwise. This variable controls for risk preferences if males are less risk-averse than females;

specialty$_i$ = indicator variable specifying which of 29 different specialties the physician practices (Table 3, Panel C). The omitted specialty is family practice;

subspecialty = indicator variable that equals 1 if the physician has a sub-specialization; and

SPECIALTY DIVERSITY = (defined previously), controls for potential economies or diseconomies of scope (Pauly 1996).

The OLS regression model used in tests of H3 (Equation (3)) also controls for size, malpractice risk, and market conditions:

$$\begin{aligned} \textit{productivity} = {} & \alpha + \beta_1 100\%\ \textit{productivity} + \beta_2 \textit{50–99\% productivity} \\ & + \beta_3 \textit{50–99\% salary} + \beta_4 \textit{100\% salary} + \beta_5 \textit{clinical FTE} \\ & + \beta_6 \textit{≤2 years} + \beta_7 \textit{3–5 years} + \beta_8 \textit{≥25 years} + \beta_8 \textit{≥male} \\ & + \beta_9 \textit{≥subspecialty} + \Sigma_{i=10}^{37}\, \beta_i \textit{specialty}_i + \beta_{38} \textit{CAPTITATION} \\ & + \beta_{39} \textit{MCO PENETRATION} + \beta_{40} \textit{COMPETITION} \\ & + \beta_{41} \textit{SUBURBAN} + \beta_{42} \textit{RURAL} + \Sigma_{i=43}^{52}\, \beta_i \textit{REGION}_i, \end{aligned} \tag{3}$$

where *productivity* refers to *ln charges, charges, ln total pay*, or *total pay*.

Correlations

Tables 4 and 5 report Pearson correlations among group-level and physician-level variables, respectively. None of the correlations between group-level variables exceeds 0.30. *INTERDEPEND* is negatively correlated with *CAPITATION* ($r = -0.30$, $p < 0.01$, two-tailed) because capitation is chiefly used with primary care specialties. Only correlations among physician-level indicator variables for compensation method exceed 0.30.

V. RESULTS

Determinants of Group-Based Compensation

Table 6 results from logistic regressions examining the determinants of group-based compensation. The first column is for the full sample of 935 groups, and compares groups that share 100 percent of their proceeds to those that use any of the four individual compensation plans. The second column pertains to a reduced sample that excludes groups with compensation plans classified as "50% to 99% Productivity" or "50% to 99% Guaranteed Salary," because these plans may include team incentives along with individual incentives. Chi-squared statistics indicate that the model is a significantly ($p < 0.01$) good fit for both the full and reduced samples, and Nagelkerke R^2 statistics ($r = 0.20$ and 0.26, respectively) indicate reasonable explanatory power.

TABLE 4
Pearson Correlations among Group-Level Predictors of Compensation Method and Variation in Productivity

	GROUP COMP	*INTERDEPEND*	*SPECIALTY DIV.*	*FTE DIV.*	*EXPER. DIV*	*GENDER DIV.*	*7–10 PHYS.*	*11–20 PHYS*	*21–30 PHYS*	*> = 31 PHYS*	*LN SIZE*	*RISK*	*< = 3 YRS*	*CAPITATION*	*MCO PEN*
INTERDEPEND	0.20 (0.00)	1.00													
SPECIALTY DIVERSITY	−0.17 (0.00)	−0.06 (0.06)	1.00												
FTE DIVERSITY	−0.03 (0.00)	−0.10 (0.00)	0.00 (0.94)	1.00											
EXPERIENCE DIVERSITY	−0.09 (0.01)	−0.05 (0.11)	0.04 (0.26)	0.16 (0.00)	1.00										
GENDER DIVERSITY	−0.08 (0.02)	−0.24 (0.00)	−0.13 (0.00)	0.20 (0.00)	0.02 (0.52)	1.00									
7–10 PHYSICIANS	0.00 (0.06)	0.04 (0.28)	−0.10 (0.00)	0.03 (0.30)	0.07 (0.03)	0.02 (0.54)	1.00								
11–20 PHYSICIANS	−0.01 (0.83)	0.01 (0.67)	−0.07 (0.04)	0.11 (0.00)	0.04 (0.17)	0.03 (0.31)	−0.27 (0.00)	1.00							
21–30 PHYSICIANS	0.07 (0.04)	0.04 (0.24)	−0.11 (0.00)	0.10 (0.00)	0.02 (0.49)	0.04 (0.25)	−0.12 (0.00)	−0.10 (0.00)	1.00						

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TABLE 4 (continued)

	GROUP COMP	*INTERDEPEND*	*SPECIALTY DIV.*	*FTE DIV.*	*EXPER. DIV*	*GENDER DIV.*	*7–10 PHYS.*	*11–20 PHYS*	*21–30 PHYS*	*> = 31 PHYS*	*LN SIZE*	*RISK*	*< = 3 YRS*	*CAPITATION*	*MCO PEN*
> = 31 PHYSICIANS	−0.11	−0.08	−0.04	0.09	0.07	0.13	−0.17	−0.14	−0.06	1.00					
	(0.00)	(0.01)	(0.26)	(0.01)	(0.04)	(0.00)	(0.00)	(0.00)	(0.06)						
LN SIZE	−0.04	−0.04	−0.23	0.21	0.15	0.20	0.00	0.30	0.29	0.70	1.00				
	(0.18)	(0.28)	(0.00)	(0.00)	(0.00)	(0.00)	(0.94)	(0.00)	(0.00)	(0.00)					
RISK	0.04	−0.04	0.03	0.01	−0.04	0.01	−0.01	−0.05	−0.04	−0.03	−0.08	1.00			
	(0.38)	(0.27)	(0.29)	(0.71)	(0.23)	(0.85)	(0.71)	(0.11)	(0.25)	(0.38)	(0.02)				
< = 3 YEARS	−0.05	−0.11	0.04	0.03	0.13	0.16	0.00	0.02	−0.01	0.02	0.02	0.01	1.00		
	(0.13)	(0.00)	(0.21)	(0.30)	(0.00)	(0.00)	(0.98)	(0.54)	(0.81)	(0.57)	(0.47)	(0.75)			
CAPITATION	−0.06	−0.30	−0.10	0.10	0.07	0.19	−0.04	0.09	0.02	0.22	0.27	0.04	0.07	1.00	
	(0.05)	(0.00)	(0.00)	(0.00)	(0.04)	(0.00)	(0.27)	(0.01)	(0.55)	(0.00)	(0.00)	(0.17)	(0.04)		
MCO PENETRATION	0.06	0.07	−0.19	0.12	0.00	0.05	0.03	0.06	0.02	−0.04	0.03	0.10	−0.03	0.22	1.00
	(0.07)	(0.04)	(0.00)	(0.00)	(0.94)	(0.12)	(0.36)	(0.08)	(0.56)	(0.20)	(0.43)	(0.00)	(0.37)	(0.00)	
COMPETITION	0.07	0.11	−0.13	0.10	0.04	0.00	0.06	0.04	0.00	−0.01	0.06	0.03	−0.07	−0.01	0.29
	(0.04)	(0.00)	(0.00)	(0.00)	(0.22)	(1.00)	(0.07)	(0.22)	(0.92)	(0.66)	(0.09)	(0.31)	(0.03)	(0.65)	(0.00)

Sample consists of 935 groups; correlations with *CAPITATION* are based on 924 groups due to missing data.
Two-tailed p-values are in parentheses.
Variables are defined in Table 1.

TABLE 5
Pearson Correlations among Physician-Level Predictors of Physician Productivity

	group comp	*100% salary*	*50–99% salary*	*50–99% productivity*	*100% productivity*	*Clinical FTE*	*< = 2 years*	*3–5 years*	*> = 25 years*	*male*
100% salary	−0.12 (0.00)	1.00								
50–99% salary	−0.16 (0.00)	−0.18 (0.00)	1.00							
50–99% productivity	−0.19 (0.00)	−0.21 (0.00)	−0.27 (0.00)	1.00						
100% productivity	−0.24 (0.00)	−0.27 (0.00)	−0.36 (0.00)	−0.42 (0.00)	1.00					
clinical FTE	0.03 (0.01)	0.12 (0.00)	0.02 (0.04)	0.04 (0.00)	0.01 (0.19)	1.00				
< = 2 years	−0.07 (0.00)	0.20 (0.00)	0.04 (0.00)	−0.06 (0.00)	−0.07 (0.00)	−0.02 (0.02)	1.00			
3–5 years	−0.05 (0.00)	0.05 (0.00)	0.06 (0.00)	−0.03 (0.01)	−0.03 (0.01)	0.01 (0.12)	−0.11 (0.00)	1.00		
> = 25 years	−0.03 (0.00)	−0.02 (0.05)	−0.05 (0.00)	0.02 (0.03)	0.06 (0.00)	−0.10 (0.00)	−0.10 (0.00)	−0.13 (0.00)	1.00	
male	0.07 (0.00)	−0.07 (0.00)	−0.10 (0.00)	0.05 (0.00)	0.04 (0.00)	0.14 (0.00)	−0.10 (0.00)	−0.09 (0.00)	0.12 (0.00)	1.00
subspecialty	−0.01 (0.28)	0.00 (0.71)	−0.02 (0.03)	0.02 (0.01)	0.00 (0.76)	0.02 (0.06)	0.01 (0.39)	0.02 (0.04)	0.00 (0.70)	0.10 (0.00)

Sample consists of 11,234 physicians.
Two-tailed p-values are in parentheses.
Variables are defined in Table 1.

TABLE 6
Group-Level Logistic Regression of Compensation Method (*GROUP COMP*) on Proxies for Task Interdependence, Intra-Group Diversity, Litigation Risk, and Size

Variables	**Predicted Sign**	**Full Sample**		**Exclude Groups Using 50%–99% Guaranteed Salary or 50%–99% Productivity**	
		Coefficient (z-statistic)	**Odds Ratio**	**Coefficient (z-statistic)**	**Odds Ratio**
Constant		−1.17 (−1.25)	0.31	1.29** (2.03)	3.64
Task Interdependence					
INTERDEPEND	+	0.40*** (4.17)	1.50	0.42*** (3.94)	1.53
Risk					
RISK	+	0.55** (1.76)	1.74	0.86*** (2.47)	2.36
Heterogeneity					
SPECIALTY DIVERSITY	−	−3.36*** (−4.29)	0.03	−3.08*** (−3.73)	0.05
FTE DIVERSITY	−	0.12 (0.11)	1.13	−0.21 (−0.18)	0.81
EXPERIENCE DIVERSITY	−	−0.09*** (−2.48)	0.92	−0.11*** (−2.94)	0.90
GENDER DIVERSITY	−	−1.60** (−1.89)	0.20	−1.67** (−1.76)	0.19
Size					
7–10 PHYSICIANS		0.09 (0.36)	1.09	0.12 (0.44)	1.12
11–20 PHYSICIANS		−0.08 (−0.29)	0.92	0.00 (0.00)	1.00
21–30 PHYSICIANS		0.30 (0.68)	1.35	0.82 (1.51)	2.27
> = 31 PHYSICIANS		−2.01*** (−2.61)	0.13	−2.10*** (−2.63)	0.12
Control Variables					
% > = 3 YEARS		0.00 (0.16)	1.00	0.00 (0.44)	1.00
MCO PENETRATION		−0.31 (−0.43)	0.73	−1.02 (−1.21)	0.36
COMPETE		0.12 (0.20)	1.12	0.35 (0.49)	1.41
SUBURB		−0.31 (−1.45)	0.74	−0.40* (−1.69)	0.67
RURAL		−1.92** (−2.49)	0.15	−2.03** (−2.48)	0.13
ROCKY MOUNTAIN		−1.31** (−2.53)	0.27	−1.55*** (−2.70)	0.21
NORTHWEST		−1.02** (−2.04)	0.36	−1.24** (−2.23)	0.29

(*continued on next page*)

TABLE 6 (continued)

		Full Sample		Exclude Groups Using 50%–99% Guaranteed Salary or 50%–99% Productivity	
Variables	**Predicted Sign**	**Coefficient (z-statistic)**	**Odds Ratio**	**Coefficient (z-statistic)**	**Odds Ratio**
SOUTHWEST		−1.27***	0.28	−1.44***	0.24
		(−3.03)		(−3.03)	
EASTERN MIDWEST		−0.73*	0.48	−1.04**	0.35
		(−1.74)		(−2.20)	
LOWER MIDWEST		−0.59	0.55	−0.96**	0.38
		(−1.41)		(−2.05)	
n *(GROUP COMP = 0)*		785		416	
n *(GROUP COMP = 1)*		150		150	
n (all groups)		935		566	
Chi-Square Statistic		117.53		114.1	
(significance)		(0.001)		(0.001)	
Nagelkerke R^2		0.202		0.266	
Correct Classification		0.842		0.769	

*, **, *** Indicate that coefficient is significance at the 10 percent, 5 percent, and 1 percent levels, respectively (one-tailed where coefficient sign has prediction, two-tailed otherwise). Only statistically significant regional control variables are reported in the table.

Variables are defined in Table 1.

Overall, the results are highly consistent with H1a–H1d.[15] In both the full and reduced sample, *INTERDEPEND* is significantly and positively associated with the use of team-based compensation ($p < 0.01$, one-tailed), suggesting that physicians in more task-interdependent specialties are more likely to use team incentives. The positive and significant coefficient on *RISK* in both the full and reduced samples ($p < 0.05$ and $p < 0.01$, respectively, one-tailed) indicates that groups facing greater malpractice risk are more likely to use team incentives. In both samples, three of the four proxies for intra-group diversity support the prediction that more diverse groups are less likely to adopt team-based compensation plans. Specialty, experience and gender diversity are negatively and significantly ($p < 0.01$, $p < 0.01$, and $p < 0.05$, respectively, one-tailed) associated with the use of group-based compensation, while the coefficient on *FTE DIVERSITY* is not significant, possibly due to the lack of variation in this variable.

Results regarding group size are consistent with H1d, in that groups with more than 30 members are significantly ($p < 0.01$, one-tailed) less likely to adopt equal-sharing plans than those with fewer than seven members. However, group size has no significant effect on the choice of compensation plan in groups containing 30 or fewer members. These results differ from those of Encinosa et al. (1997), who find that medical groups are significantly less likely to use equal-sharing arrangements as group size increases beyond five members. The difference is likely attributable to substantial increases in income risk and significant reductions in monitoring costs that have taken place during the 20 years that have elapsed between the time the two surveys were conducted (Mauney 2002; Leone 2002; Casalino et al. 2003).[16]

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[15] I tested the robustness of the results with alternative specifications of the model in Tables 6 (Equation (1)). I re-estimated the model after replacing *INTERDEPEND* with a set of 17 indicator variables representing a group's specialty. The untabulated results were materially consistent with those contained in Table 6. I also re-estimated the model using states as control variables in place of the regional control variables because tort reform regulation and liability payment limits vary by state. Inclusion of state control variables did not significantly improve the model's fit. The size of the coefficient for *RISK* increased (from 0.55 to 1.10) and the significance dropped slightly (from $p = 0.039$ to $p = 0.047$). Finally, controlling for capitation (*CAPITATION)* did not materially affect the results and capitation revenues were not significantly associated with compensation method. Including this control variable reduced the number of observations to 924; therefore, it is not included in the final model.

Adoption of equal-sharing plans by groups with fewer than 30 members is consistent with agency-based predictions that free-riding and constraints on monitoring capabilities limit the use of group incentives in very large groups. An alternative explanation is that the marginal benefits of cooperation decrease as group size increases, thereby reducing the impetus for groups to use team incentives to induce cooperation. To the extent that decreasing marginal benefits are associated with the absolute size of cooperative benefits, *INTERDEPEND* controls for decreasing marginal returns to size.[17]

As with most cross-sectional compensation studies, the estimation results presented in Table 6 will potentially be confounded if the regressor variables are endogenous choices by the practice. However, if the regressor variables are exogenous or predetermined at the time the practice selects a compensation plan, then endogeneity should not affect the results. Prior research suggests this is the case. Ittner et al. (2007, 315) argue that groups generally choose location and specialty or specialties upon formation, and most operate for many years. Location and specialty, in turn, determine the group's competitive market, legal environment, group size, and management practices (Pauly 1996; Kessler and McClellan 2000; Casalino et al. 2003).[18]

Physician experience is clearly predetermined, and there is no evidence that physicians select into compensation arrangements based upon their own experience or the experience levels of other group members (Encinosa et al. 1997; Gaynor and Pauly 1990). It is plausible that if women are more risk-averse than men, they will seek out practices with equal-sharing arrangements upon entering the workforce. However, as previously discussed, the significant positive correlation between *GENDER DIVERSITY* and percentage of women in a group will bias tests against showing support for H1c.

One regressor variable that is not clearly exogenous or predetermined is *FTE DIVERSITY,* which measures the variation in the proportion of FTE units each group member devotes to clinical activity. Although endogeneity is not likely to be a concern with *FTE DIVERSITY*, it is plausible that compensation method influences the amount of time each physician devotes to clinical activity. Physicians who are compensated based on individual clinical productivity clearly have a very strong incentive to devote time purely to clinical activities, as opposed to teaching residents or performing administrative tasks. However, *FTE DIVERSITY* is not significantly associated with group-based compensation.

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[16] As income risk increases, group size must increase correspondingly to provide the same level of insurance in 1998 that existed in a smaller group in 1978. Substantial improvements in medical billing and information technology have made it easier to monitor a greater number of physicians.

[17] Moreover, median group sizes for all specialties are well below 30 physicians, the point in this sample at which medical groups are significantly less likely to use team incentives. If marginal benefits to cooperation drive the use of equal-sharing, it seems unlikely that these benefits would decline at a size that is significantly larger than the median group size.

[18] It is unlikely that groups simultaneously choose compensation plans and locations upon formation because there are significant barriers to the formation of a new group in a new location. Physicians cannot move their patients with them. At the same time, the group must incur the normal relocation costs of any business. Upon entering the workforce, physicians generally join established groups or take over practices of solo practitioners who are retiring in order to gain access to an existing patient base and existing administrative systems. Movement of physicians to new practices is limited by non-compete clauses, the substantial relocation costs, and a physician's ownership stake in a medical group.

Finally, practices with higher concentrations of uninsured and Medicaid patients are subject to greater financial risk than those with wealthier patient panels. Accordingly, practices with relatively poor patient panels should prefer equal-sharing plans to reduce each individual physician's exposure to financial risk. In untabulated robustness tests, I control for clientele wealth effects with a group-level measure of total adjustments to gross charges. Adjustments equal the difference between gross charges (amounts billed) and net revenues (amounts collected) and include reductions for Medicare fee restrictions, contractual adjustments, bad debts, and other adjustments. Fewer adjustments suggest less financial risk. Including adjustments in the model (Table 8) reduces the sample size from 935 to 699 groups, but does not materially alter the results or conclusion.

Group-Based Compensation and Intra-Firm Variation in Productivity

Table 7 reports ordinary least-squares estimates for tests of H2. As predicted, intra-group variation in gross charges is significantly lower in practices that use equal-sharing arrangements when compared with those that use individual compensation plans ($p < 0.01$, two-tailed). Unfortunately, the data do not permit me to distinguish whether this result is attributable to selection and/or incentive effects. Moreover, if homogenous groups tend to choose equal-sharing rules and to adopt standardized operating procedures, intra-group homogeneity will lead to lower productivity variances. Measures of homogeneity and size are included in the model to help control for the relation between intra-group homogeneity and the use of standardized procedures. The *INTERDEPEND* measure also controls for the adoption of standardized operating policies if specialties with greater task interdependence are more likely to adopt standardized procedures.

Group-Based Compensation and Productivity

Table 8 reports regression estimates for tests of H3. Observations from physicians in the same group are unlikely to be independent. Therefore, I calculate standard errors using a robust variance estimator, which does not assume that observations within the same medical group are independent.[19] This estimator assumes only that observations from different groups are independent of each other. R^2 statistics are 0.60 and 0.57 for *ln charges* and *ln total pay*, respectively. There is no significant difference between the amount of charges generated by physicians in group-based compensation plans and those in individual compensation plans of any type. However, the relation between total pay and compensation plan depends upon the size of the productivity component in the individual plan. The models using *charges* and *total pay* as the dependent variables predict that salaried physicians and those who receive 50 to 99 percent of their compensation as salary earn $66,983 and $26,208 less, respectively, than those in equal-sharing plans ($p < 0.01$, two-tailed). Total

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[19] The robust variance estimator in Table 8 is an extension of the standard Huber (1967)/White (1980)/sandwich estimator of variance and corrects for clustering of observations by group (StataCorp 2001, 254–258). I also estimated the relation between compensation and productivity with fixed-effects and random-effects models. Results indicated that physicians in equal-sharing arrangements generate significantly more charges than those on individual plans with salary components that exceed 50 percent; in addition, they earned significantly more income than physicians in individual plans. However, the fixed-effects model fails to capture the effect of group-level variables, many of which are significantly associated with productivity and compensation method. The random-effects model includes all group-level variables; however, a Hausman test of the coefficients suggests that the model is misspecified. Together, the OLS, the fixed-effects, and the random-effects models indicate that physicians in equal-sharing arrangements are at least as productive as those in individual plans.

TABLE 7

Group-Level OLS Regression of the Intra-Group Variation in Charges (*CHARGE VARIATION*) on Use of Group-Based Compensation and Controls Variables

Variables	Predicted Sign	Coefficient (t-statistic)
Constant		−0.05
		(−1.33)
GROUP COMP	−	−0.06***
		(−3.68)
Control Variables		
INTERDEPEND		−0.02***
		(−3.14)
SPECIALTY DIVERSITY		0.27***
		(8.19)
FTE DIVERSITY		0.70***
		(12.63)
EXPERIENCE DIVERSITY		0.01***
		(3.72)
GENDER DIVERSITY		−0.05
		(1.09)
LN SIZE		0.10***
		(13.47)
RISK		0.00
		(−0.05)
CAPITATION		−0.02***
		(−3.05)
MCO PENETRATION		−0.02
		(−0.45)
COMPETITION		0.00
		(0.15)
NO. CALIFORNIA		0.09*
		(1.90)
n (groups)		856
F		27.23
(significance)		(0.000)
Adjusted R^2		0.414

*, **, *** Indicate that coefficient is significance at the 10 percent, 5 percent, and 1 percent levels, respectively, (one-tailed where coefficient sign has prediction, two-tailed otherwise).

Only significant control variables for metropolitan area and geographic region are reported in the table.

Variables are defined in Table 1.

pay does not differ between physicians in equal-sharing arrangements and those in individual plans with a productivity component of at least 50 percent.

Taken together, the results on the determinants and performance effects of group incentives are consistent with two possible agency-based explanations. First, when group tasks are highly interdependent, firms use team incentives to induce cooperation, thereby generating productive benefits sufficient to offset losses from free-riding. Second, group-based compensation arrangements in homogenous groups encourage a level of low-cost mutual monitoring that is sufficient to curb free-riding. Unfortunately, the data do not permit me to distinguish between the effects of cooperation and mutual monitoring on productivity.

TABLE 8
Physician-Level Regression of Gross Charges and Total Pay on Compensation Method and Control Variables for Physician Characteristics, Group Characteristics, and Market Conditions with Robust Standard Errors and Clustering on Physician Group[a]

	ln charges	*charges*	*ln total pay*	*total pay*
Variables	**Coefficient (t-statistic)**	**Coefficient (t-statistic)**	**Coefficient (t-statistic)**	**Coefficient (t-statistic)**
Constant	13.03***	502,011***	12.21***	367,140***
	(168.17)	(5.90)	(186.66)	(11.30)
Compensation Method				
100% productivity	0.02	58,159	−0.04	3,037
	(0.59)	(1.12)	(−1.04)	(0.21)
50–99% productivity	0.01	38,674	−0.03	−11,295
	(0.28)	(0.73)	(−0.91)	(−0.78)
50–99% salary	−0.05	−15,208	−0.08**	−26,208*
	(−1.19)	(−0.32)	(−2.24)	(−1.85)
100% salary	−0.07	82,708	−0.27***	−65,983***
	(−1.21)	(0.62)	(−7.58)	(−4.92)
Control Variables[b]				
Physician Characteristics				
clinical FTE	1.98***	894,762***	1.60***	194,936***
	(25.08)	(11.70)	(19.41)	(15.62)
< = 2 years	−0.23***	−198,672***	−0.33***	−78,983***
	(−8.38)	(−5.17)	(−14.12)	(−12.18)
3–5 years	−0.06**	−58,822**	−0.15***	−39,426***
	(−3.60)	(−2.14)	(−10.92)	(−9.05)
> 25 years	−0.23***	−190,222***	−0.18***	−46,260***
	(−11.62)	(−10.37)	(−10.45)	(−9.81)
male	0.10***	51,074	0.15***	27,016***
	(5.41)	(1.58)	(10.60)	(9.12)
subspecialty	0.15***	232,549***	0.12***	53,944***
	(4.24)	(5.23)	(4.22)	(4.89)
Group Characteristics				
SPECIALTY DIVERSITY	−0.28**	−235,556**	−0.37***	−104,418***
	(−3.23)	(−2.42)	(−5.45)	(−4.21)
LN SIZE	−0.05***	−40,633***	−0.04***	−15,043***
	(−4.46)	(−3.07)	(−3.74)	(−4.70)
Market Conditions				
RISK	0.11**	100,601*	0.07**	29,442**
	(2.60)	(1.84)	(2.07)	(2.27)
CAPITATION	−0.01	−11,234	−0.02**	−5,019**
	(−0.72)	(−1.02)	(−2.40)	(−2.31)
MCO PENETRATION	0.01	86,540	−0.05	−34,675
	(0.06)	(0.88)	(−0.71)	(−1.39)
COMPETITION	0.00	−37,965	−0.08	−29,247*
	(−0.03)	(−0.33)	(−1.41)	(−1.68)
RURAL	−0.11**	−67,941	−0.10**	−30,136***
	(−2.05)	(−1.49)	(−2.50)	(−2.92)
SUBURB	0.00	14,490	−0.01	−5,866
	(0.05)	(0.49)	(−0.64)	(−0.81)
n (physicians)	10,074	10,074	11,234	11,234
n (groups)	853	853	919	919

(continued on next page)

TABLE 8 (continued)

	ln charges	*charges*	*ln total pay*	*total pay*
Variables	**Coefficient (t-statistic)**	**Coefficient (t-statistic)**	**Coefficient (t-statistic)**	**Coefficient (t-statistic)**
F	81.25	44.88	75.53	35.71
(significance)	(0.000)	(0.000)	(0.000)	(0.000)
Adjusted R^2	0.600	0.361	0.571	0.429

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*, **, *** Indicate that coefficient is significance at the 10 percent, 5 percent, and 1 percent levels, respectively, (two-tailed).

[a] Standard errors are calculated using a robust variance estimator that does not treat all observations as independent, but does assume that observations can be divided into M groups, $G1, G2, \ldots, GM$, that are independent. The formula for this calculation is below:

[b] The regression model controls for region by including 11 indicator variables; and, it controls individual physician specialty by including 28 different indicator variables (Table 3, Panel C). However, coefficients for region and specialty are not reported in the table.

$$\hat{v} = \hat{V}\left(\sum_{k=1}^{M} u_k^{(G)\prime} u_k^{(G)}\right)\hat{V}$$

where:

$$\hat{V} = (-\partial^2 \ln L / \partial \ln B^2)^{-1}$$

and:

$$u_k^{(G)} = \sum_{j \in G_k} u_j.$$

However, both task interdependence and homogeneity are positively associated with the use of team incentives, suggesting that both cooperation and monitoring affect productivity.

The results are also consistent with the social categorization perspective of diversity, which holds that groups function more smoothly when they are homogeneous (Williams and O'Reilly 1998). However, extant research shows mixed support for the performance effects of intra-group homogeneity (see Van Knippenberg and Schippers [2007] for a review). Furthermore, controlling for diversity in the robustness tests of the productivity regressions does not materially affect the results.

My results differ from prior studies, which used a 1978 data set of physician group practices (Gaynor and Pauly 1990; Gaynor and Gertler 1995; Encinosa et al. 1997). Encinosa et al. (1997) found that equal-sharing groups with more than five members averaged significantly fewer patient visits than groups that did not use equal-sharing, while I find no productivity differences in groups that have as many as 30 members. Substantial changes in the organization of medical group practices and the health care environment likely account for the difference in results. In particular, the level of task interdependence in health care delivery has increased and improvements in information technology have lowered monitoring costs (Casalino et al. 2003; Levinson and Lurie 2004). Differences in the specialty composition of the samples may also contribute to the contrasting results. Encinosa et al.'s (1997) sample does not include hospital-based specialties, which tend to practice in relatively large groups and use group incentives.

VI. CONCLUSION

This study makes several important contributions to the extant literature by providing large-sample, empirical evidence on (1) the role of task interdependence, income risk, mutual monitoring, and size in a firm's choice of compensation contract, and (2) individuals' responses to group incentives. First, I develop measures of task interdependence for different physician specialties and find that differences in task interdependence are associated with differences in compensation plans in a manner consistent with agency theory. Prior studies have established technological differences across work groups (e.g., Wageman 1995; Wageman and Baker 1997), but none has found empirical support for the premise that firms design incentive systems to match task interdependency. These results are also related to accounting research that examines the influence of organizational interdependencies on compensation contract design (Bushman et al. 1995; Keating 1997). Second, this study provides empirical support for research that models partnerships as insurance devices and thus helps explain the existence of limited liability partnerships (Gaynor and Gertler 1995; Lang and Gordon 1995). The current results also complement Evans et al. (2006), who find that legal risk influences physician contracts with MCOs. Third, this study measures mutual monitoring costs, as opposed to external monitoring costs, and relates these costs to contract design. Findings are consistent with the premise that firms choose equal-sharing arrangements when homogeneity in training, age, and gender facilitates mutual monitoring by group members. Finally, the study provides important evidence on the performance effects of group incentives by examining *individual,* as opposed to group, output. Consistent with prior research, firms that use group incentives exhibit significantly less variation in productivity than those that use individual plans. In contrast to prior research, I find that physicians whose compensation is based upon group performance are not less productive than those whose compensation is based upon individual performance. Accordingly, this study provides a new perspective on the performance effects of group incentives in labor-managed firms by using a larger, more recent, and more comprehensive data set than that used in prior research. Consistent with recent theoretical research (e.g., Holmstrom and Milgrom 1991; Arya et al. 1997; Che and Yoo 2001), the findings indicate that physician firms choose group-based compensation when team incentives are effective in inducing levels of cooperation and mutual monitoring that are sufficient to offset and/or prevent free-riding.

The results are potentially useful for other professional firms, such as law, consulting, and accounting, which share key organizational characteristics with medical practices. In particular, professionals are typically organized into labor-managed firms, and most perform work that varies in the level of task interdependence. Furthermore, all professionals generally make significant investments in their own human capital, and the quality and quantity of inputs (e.g., hours) are easily monitored in many professional service firms (Hansmann 1996).

These results are also of interest to managers and owners of corporations who increasingly rely upon employees to work in teams to accomplish goals. Although corporations differ fundamentally from labor-managed firms in governance and organization, compensation committees and managers should still evaluate task interdependence, mutual monitoring effectiveness, and group size in determining the extent to which they use group incentives to compensate employees.

This study is subject to a number of limitations. As with prior compensation research, the data exclude a number of direct and indirect compensation sources, such as pensions, insurance, perquisites, and future capital gains from the sale of partnership stakes. Cross-sectional studies such as this can establish associations, but not causality. While my results are consistent with agency theory, they are also consistent with alternative explanations. Where possible, I have included control variables to address competing explanations, but I cannot completely rule them out. Moreover, I am unable to distinguish between the performance effects of different agency-based constructs, such as mutual monitoring and free-riding, nor can I distinguish between the effects of incentives and selection on performance. Future studies using panel data would enable researchers to investigate the performance effects of introducing and discontinuing group-based compensation plans. Finally, analysis of professionals in the single field of medicine limits the ability to generalize to other professions and work settings. Despite these limitations, the study expands the literature on group incentives by providing large-sample empirical tests of agency-based predictions.

APPENDIX
DETERMINATION OF TASK INTERDEPENDENCE MEASURE

The task interdependence measure (denoted *interdepend*) is based on a survey of 18 physicians in executive education programs at The University of Texas at Dallas. Physicians rated each specialty (using a scale of 1 to 5) on the following three dimensions: (1) the importance of maintaining continuity in care (denoted *continuity*), (2) the amount of individual influence a physician exerts over his patient panel (denoted *panel influence*), and (3) the extent to which a physician controls the timing and amount of service provided (denoted *schedule control*). Table A-1 contains the survey results. Responses to each survey question have been reverse-coded so that lower numbers indicate that continuity is of greater importance and that physicians have greater patient influence and greater schedule control. Hence, higher ratings indicate greater benefit from intra-group cooperation and teamwork. The variable *interdepend* is an average of the three reverse-coded responses for *continuity*, *panel influence*, and *schedule control*.

Specialties in Table A-1 have been grouped into one of five major medical categories: primary care, hospital-based, surgical, internal medicine subspecialty, and other. The results are highly consistent with the practitioner literature on medical specialties. For example, consistent with Nutting et al. (2003) and Saultz (2003), the physician survey respondents rated continuity of care to be most important in the primary care specialties (means range from 1.17 to 1.73). Physicians rated continuity to be least important for hospital-based specialists (means range from 4.67 to 5.00), who typically treat patients on a nonrecurring and urgent basis (Taylor 2003). As predicted by Schwartz et al. (1990), survey respondents indicated that plastic surgeons, ophthalmologists, dermatologists, and psychiatrists exert the greatest influence on their patient panels and have the greatest control over their work schedules.

TABLE A-1
Mean and Median Responses to Physician Survey Measuring Task Interdependence[a,b]

	n-groups	*Continuity*		*Panel Influence*		*Schedule Control*		*Interdepend*[c]	
		Mean	**Median**	**Mean**	**Median**	**Mean**	**Median**	**Mean**	**Median**
Primary Care									
Family Practice	129	1.20	1	2.07	2	2.86	3	1.98	1.67
Internal Medicine	89	1.17	1	2.12	2	3.00	3	2.04	2.00
Pediatrics	34	1.40	1	2.29	2	3.14	3.5	2.20	2.00
Obstetrics-Gynecology	66	1.73*	2*	2.64**	3**	4.07***	4.5**	2.76***	2.67***
Hospital-Based									
Anesthesiology	43	4.67***	5***	3.57***	4***	2.14	2	3.56***	3.33***
Emergency Medicine	5	4.80***	5***	4.57***	5***	1.71***	1**	3.78***	3.67***
Pathology	7	5.00***	5***	3.86***	4.5***	1.36***	1***	3.51***	3.67***
Radiology	27	4.93***	5***	3.93***	4.5***	1.57***	1.5***	3.58***	3.67***
Surgical									
General Surgery	32	3.11***	3***	2.93***	3***	3.57*	4*	3.21***	3.33***
Vascular Surgery	7	3.43***	4***	2.79***	3***	3.21	3	3.17***	3.33***
Cardiothoracic Surgery	21	3.47***	4***	2.93***	3***	3.47*	4*	3.30***	3.33***
Neurological Surgery	22	3.34***	4***	2.93***	3***	3.40	4	3.24***	3.33***
Orthopedic Surgery	151	3.30***	3.5***	2.86**	3**	2.93	3	3.06***	3.00***
Plastic Surgery	6	3.57***	4***	1.86	2	1.86**	1.5**	2.50*	2.33*
Ophthalmology	37	3.17***	3***	2.43	2.5	1.64***	2***	2.48**	2.33***
Otolaryngology	22	3.37***	3***	2.71***	3**	2.50	2	2.90***	3.00***
Urology	39	3.37***	3***	2.43	2.5	2.43	2.5	2.79***	2.67***

(*continued on next page*)

TABLE A-1 (continued)

	n-groups	*Continuity* Mean	*Continuity* Median	*Panel Influence* Mean	*Panel Influence* Median	*Schedule Control* Mean	*Schedule Control* Median	*Interdepend*[c] Mean	*Interdepend*[c] Median
Internal Medicine Subspecialty									
Cardiology	94	1.80***	2***	2.21	2.5	3.14	3	2.36**	2.00**
Gastroenterology	24	2.70***	3***	2.50	3	2.50	2.5	2.57***	2.67***
Hematology-oncology	18	1.67**	2**	2.43	2.5	2.50	2	2.13	2.00
Nephrology	13	1.84***	2***	2.60**	3**	2.67	3	2.36***	2.42**
Pulmonary medicine	11	2.27***	2***	2.79**	3**	2.86	3	2.60***	2.67**
Other									
Allergy	12	2.63***	3***	1.86	1.5	1.29***	1***	1.97	2.00
Neurology	13	2.47***	2.25***	2.87***	3***	2.20**	2**	2.51***	2.58***
Dermatology	8	2.80***	3***	1.50**	1**	1.14***	1***	1.89	1.67
Physiatry	4	2.87***	3***	2.50	2.5	1.43***	1***	2.27	2.33
Psychiatry	1	1.73*	2*	1.86	2	1.50***	1***	1.67*	1.67*
Total	935								

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*, **, *** Indicate p-values of 0.01, 0.05, and 0.10, respectively for tests of equal means (medians).

[a] Mean and median physician responses to questions in Task Interdependence Survey. *Continuity—How important is continuity of care in this specialty? Influence patient—To what extent can a physician in this specialty individually influence the size and/or nature of his/her patient panel or case load? Control schedule—To what extent can a physician in this specialty control when and how many hours are worked in a given week?* Responses are on a scale of 1–5 and have been coded so that lower numbers indicate that *continuity* is of greater importance and that physicians have greater *patient influence* and greater *schedule control.* Hence, higher ratings indicate greater benefit from intra-group cooperation and teamwork. Mean and median values computed from 14–18 responses per variable.

[b] Means (medians) for each variable for each specialty (except family practice) were compared to the corresponding mean (medians) for family practice using a paired t-test (signed rank test) for the 14 respondents who ranked each pair.

[c] *task interdependence* is the mean of each physician's responses to question on *continuity, patient influence,* and *schedule control.* Higher numbers indicate greater benefit from intra-group cooperation and teamwork.

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BOOK REVIEWS

Stephen A. Zeff, Editor

Editor's note: Two copies of books for review should be sent to the Book Review Editor: Stephen A. Zeff, Rice University, Jesse H. Jones Graduate School of Business, 6100 Main St., Houston, TX 77005. The policy of *The Accounting Review* is to publish only those reviews solicited by the Book Review Editor. Unsolicited reviews will not be accepted.

MARY E. BARTH, *Research, Standard Setting, and Global Financial Reporting*, Foundations and Trends© in Accounting (Hanover, MA: now Publishers Inc., 2006, ISBN: 978-1-60198-008-3, vol. 1, no. 2, pp. x, 99).

Mary Barth is unique among academics in having worked as an audit partner for Arthur Andersen, served on the International Accounting Standards Board, and produced as much policy-oriented research as anyone in her generation. As a consequence, she is well qualified to provide insights into the relation between research and standard setting in a global context.

The contribution of the book is not so much in breaking new conceptual ground as in providing a cohesive discussion of the goals of standard-setting research, the inherent opportunities and challenges in designing tests to inform regulators, examples from existing research, and opportunities going forward. I envision the book as being a natural fit for use in both Ph.D. seminars and general research colloquia on empirical research, as well as for more senior researchers interested in targeting their work specifically to standard setting.

The book begins by laying out the distinction between "motivating questions" (questions of interest to standard setters), and "research questions" (questions that researchers attempt to address). Mary argues that many questions of interest to standard setters do not necessarily lend themselves to traditional research approaches, particularly normative questions around optimal standards. Similarly, research questions can have a variety of potential audiences, and only a subset is targeted to standard setters. As a result, if the researcher has chosen to undertake policy-oriented research, it is crucial to have strong grounding in both the standard-setting and research literatures to develop questions that are relevant to standard setters as well as feasible from a research design perspective.

The book then transitions to a discussion of the debate over the role of research in standard setting. I found this section to be particularly interesting and relevant for a Ph.D. course on empirical research. Mary does a very clear and balanced job of laying out the strengths and weaknesses of research for addressing standard-setting issues. She is particularly well suited to do so, given her role in the debate over the place of value-relevance research in standard setting. She acknowledges that research cannot ultimately determine optimal accounting rules, given the trade-offs between the interests of various constituencies, but emphasizes that research can inform standard setting conditional on the objectives stated by the standard setter. For example, research has a harder time identifying whether representational faithfulness should be an important attribute of financial reporting, but, given that standard setters identify it as such, researchers can provide relevant descriptive evidence on which accounting amounts are more highly associated with prices. Similarly, researchers can provide evidence on information content, given that standard setters have identified informativeness as an important attribute of reporting. However, research can probably not settle the underlying optimality question of the appropriate tradeoff between information content and representational faithfulness.

This section also includes a useful historical perspective on the role of research in standard setting, beginning with Gonedes and Dopuch (1974), and focusing on the debate between Barth et al. (2001) and Holthausen and Watts (2001). The discussion highlights the importance of appropriate caution in interpreting the implications of empirical research for standard setting.

The next section provides a more detailed discussion of the strengths and weaknesses of various research approaches in designing empirical tests, focusing on capital markets research. This plays to Mary's strengths, because she has a strong econometrics background and has been a significant contributor to the literature on experimental design. She begins with a basic valuation model and develops the implications for tests based on levels versus changes specifications, and on event studies. While the basic concepts are not new, Mary provides a clear analysis and synthesis of various approaches. There is relatively little emphasis on empirical approaches that do not rely on share price, or on analyses based on alternate methods, such as experiments or analytical modeling. However, Mary is clear that she intentionally limits the discussion to her areas of expertise, and that provides needed focus to the book.

Having developed the theoretical underpinnings, the book provides five examples of Mary's research on issues related to fair value accounting, illustrating the approach that Mary uses to identify relevant motivating questions and to develop appropriate research designs. This section links nicely to the preceding conceptual development in providing concrete examples. While the presentation of each paper is brief, I found it particularly helpful that the papers were around a common theme; it was easy to see how a general question could be broken down into manageable sub-questions and developed by using appropriate research designs.

In the fifth section, Mary provides a list of 63 potential motivating questions for future research across eight areas of primary concern to standard setters (e.g., consolidations, revenue recognition, and financial statement presentation). As much as anything, the list shows, from the standpoint of a standard setter, how much remains to be done. Certainly, the list would provide ample fodder for discussion in a Ph.D. seminar and help to refute the impression that "it's all been done already." While there are design challenges underlying many of the questions, it is hard to imagine leaving the list without some idea of potential research topics.

Finally, Mary highlights the role for research on global reporting issues. Since many of the topics to this point are inherently global, the focus is on understanding the issues that arise when one moves from country-specific standards to standards designed to serve across boundaries. The discussion is fairly brief, but it highlights some of the emerging issues that the IASB faces in continuing to develop a converged set of standards designed to apply across a wide range of settings.

Overall, Mary does an excellent job of developing both the theoretical underpinnings of research on standard setting, as well as providing specific examples from the literature and avenues for future research. The book should appeal to a variety of audiences, including Ph.D. students, research faculty, and even standard setters.

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JENNIFER FRANCIS, PER OLSSON, and KATHERINE SCHIPPER, *Earnings Quality*, Foundations and Trends© in Accounting, (Hanover, MA: now Publishers Inc., 2008, ISBN: 978-1-60198-114-1, vol. 1, no. 4, pp. x, 85).

Earnings Quality, a monograph by Jennifer Francis, Per Olsson, and Katherine Schipper (hereafter, FOS), is the fourth issue in the Foundations and Trends© in Accounting series. The monograph provides a valuable perspective on earnings quality research in a capital market setting for accounting researchers and doctoral students. The focus on capital market implications is a distinctive feature of this work; the content is complementary to the recent monograph on earnings quality by Dechow and Schrand (2004), who take a financial statement analysis perspective. FOS develop a framework for interpreting research on central questions in this literature and discuss alternative constructs and measures of earnings quality and research design choices. The authors draw on the extensive set of studies they conducted as well as the related broader literature. In addition, the monograph suggests opportunities for further research on earnings quality in capital market and other settings.

In the Abstract, FOS motivate their focus on earnings quality in a capital market setting explicitly:

> capital market uses of accounting information are fundamental, in the sense of providing a basis for other uses, such as stewardship. Because resource allocations are *ex ante* decisions while contracting/stewardship assessments are *ex post* evaluations of outcomes, evidence on whether, how and to what degree earnings quality influences capital market resource allocation decisions is fundamental to understanding why and how accounting matters to investors and others. . . .

Underlying this focus is an interest in understanding whether higher earnings quality provides benefits to companies or their investors. As FOS note, they do not focus on earnings quality in contracting and stewardship settings, but I believe their discussion of earnings quality constructs, measures, and relations with capital market outcomes would provide beneficial background to researchers studying earnings quality in these contexts as well. Research focused on contracting and stewardship settings also offers promise, as it is possible that the capital market consequences of earnings quality arise in part because investors derive assurance from earnings quality about the management of contracting or agency problems.

In Chapter 2, FOS define information quality as "the precision of a measure with respect to a valuation relevant construct. For a given construct, higher quality information is more precise ... with respect to that construct" (p. 8). The authors view financial reporting quality as a special case of information quality, and they suggest higher financial reporting quality should result in higher quality judgments and decisions that result in better capital allocation decisions. They discuss the institutional characteristics of financial reporting, and provide an overview of research on many attributes, making clear that the implicit assumption in this research is that higher levels of a reporting quality construct generate more precise information.

FOS view earnings quality as a summary indicator of financial reporting quality. However, they discuss some of the challenges associated with using earnings as a summary statistic for financial reporting information, including the fact that different components of earnings may have different quality, and that balance sheets and other disclosures can offer important information. The focus on earnings quality as a construct seems to stem both from its wide applicability and from the challenges associated with measuring financial reporting quality, suggesting considerable opportunity for future work on alternative earnings and non-earnings measures of financial reporting quality.

Chapter 3 contains their model of earnings quality, in which earnings quality is affected by two types of factors: "those that reflect innate features of business models and operating environments, and those that reflect the financial reporting process *per se*" (p. 17). They also refer to the reporting factors as discretionary, although I find the reporting characterization more useful. The notion of discretionary reporting sources parallels the focus of the earnings management literature on discretionary and nondiscretionary components of accruals, where the notion is that management has discretion over the level of accruals (see McNichols and Wilson 1988). In the context of the reporting processes that affect earnings quality, the decision-maker exercising discretion could be a standard-setter, auditor, manager, audit committee, or others. Thus, for a given context and participant in the reporting process, the reporting effect of interest could be discretionary or non-discretionary.

Their framework suggests that a firm can have low-quality earnings (its earnings or accruals are a noisy signal about a value-relevant attribute) because the firm's operational characteristics result in a weak association, its reporting practices result in a weak association, or both. I believe that this is a useful characterization. In addition, it poses a substantial challenge to researchers interested in the informativeness of alternative reporting processes, because they must either identify powerful proxies for reporting processes and their consequences or use measures of total earnings quality and control for the operational effects.

FOS discuss two approaches that use a measure of total earnings quality to test hypotheses concerning reporting effects. The first approach estimates discretionary earnings quality as the prediction error from a regression of total earnings quality on measures of innate factors. The dependent variable of interest (e.g., cost of capital) is then regressed on the estimate of discretionary earnings quality. The second approach regresses a dependent variable of interest (e.g., cost of capital) on total earnings quality and the measures of innate factors, and makes inferences about reporting effects based on the coefficient on total earnings quality. In their discussion of coefficient bias in the two approaches, it was not clear what they assumed about measurement error in total earnings quality, innate earnings quality, discretionary earnings quality, or the dependent variable of interest, nor what they assumed about the correlations between these errors and the included variables. Conclusions about the direction of bias in the two approaches are sensitive to these assumptions. So it is important to make them explicit.

As the authors note, the correlation between extant measures of earnings quality and proxies for firms' operational characteristics is generally high, whereas the correlation between earnings quality and proxies for reporting factors is generally substantially lower. This is a very important finding, and it suggests that the magnitude of reporting effects is likely to be small. However, if earnings quality influences cost of capital, then a reporting choice can have both direct and indirect effects, so that the operational characteristics could be influenced by reporting choices. In other words, the consequences of reporting choices could be subsumed in the measures of operational characteristics. Relatedly, FOS note that there has been "no systematic and rigorous demonstration of the construct validity of the innate and discretionary earnings quality measures, independent of the specific research contexts in which they have been used" (p. 24). This is a valuable direction for future research.

Chapter 4 provides a comprehensive and thoughtful overview of commonly used measures of earnings quality, including their strengths and limitations in a capital market context. FOS discuss the Dechow and Dichev (2002) accruals quality measure, the Jones (1991) abnormal accruals measure, the persistence of earnings, predictability of earnings using earnings or analysts' earnings forecasts, the smoothness of earnings relative to cash flows, the variability of earnings, value relevance, the earnings response coefficient, earnings opacity, timeliness, and e-loadings. These measures rely either solely on accounting numbers, or on accounting numbers in relation to stock prices.

Chapter 5 discusses the relation between earnings quality and capital market outcomes. FOS focus primarily on cost of capital as a summary indicator of investors' resource allocation decisions. FOS interpret the extant literature as supporting an association between earnings quality and cost of capital. Their discussion of the analytical literature on earnings quality and expected returns provides insight into why accounting researchers are interested in this relation. The earlier literature hypothesized that the effects related to information quality were priced by assuming differences in information across investors. More recent literature, such as Lambert et al. (2007), develops a CAPM-consistent model where investors have homogeneous information and yet accounting quality can influence expected returns both directly and indirectly. An important direction for future research is to articulate the implications of this relation at a more fundamental level. For example, it would be interesting to understand whether the relation between earnings quality and cost of capital reflects direct implications for assessments of future cash flows, or the indirect effects of earnings quality on a firm's real investment decisions. FOS also note that, while there is an association between earnings quality and cost of capital, whether earnings quality affects expected returns through a separate information risk factor or through the risk premium or beta remains an open question for future research.

In summary, FOS provide a well-written and insightful discussion of the significance of researchers' choices of constructs, measures, and designs in the earnings quality literature. The monograph is a valuable roadmap for understanding this literature and for identifying opportunities for future research that makes alternative assumptions, focuses on related questions, or goes beyond a capital market context.

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EDITORIAL POLICY AND STYLE INFORMATION

EDITORIAL POLICY

According to the policies set by the Publications Committee (which were endorsed by the Executive Committee and were published in the *Accounting Education News*, June 1987), *The Accounting Review* "should be viewed as the premier journal for publishing articles reporting the results of accounting research and explaining and illustrating related research methodology. The scope of acceptable articles should embrace any research methodology and any accounting-related subject, as long as the articles meet the standards established for publication in the journal ... No special sections should be necessary. The primary, but not exclusive, audience should be—as it is now—academicians, graduate students, and others interested in accounting research."

The primary criterion for publication in *The Accounting Review* is the significance of the contribution an article makes to the literature. Topical areas of interest to the journal include accounting information systems, auditing and assurance services, financial accounting, management accounting, taxation, and all other areas of accounting, broadly defined. The journal is also open to all rigorous research methods.

The efficiency and effectiveness of the editorial review process is critically dependent upon the actions of both authors submitting papers and the reviewers. Authors accept the responsibility of preparing research papers at a level suitable for evaluation by independent reviewers. Such preparation, therefore, should include subjecting the manuscript to critique by colleagues and others and revising it accordingly prior to submission. The review process is not to be used as a means of obtaining feedback at early stages of developing the research.

Reviewers and editors are responsible for providing constructive and prompt evaluations of submitted research papers based on the significance of their contribution and on the rigor of analysis and presentation.

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The Accounting Review's manuscript preparation guidelines follow *The Chicago Manual of Style* (15th ed.; University of Chicago Press). Another helpful guide to usage and style is *The Elements of Style*, by William Strunk, Jr., and E. B. White (Macmillan). Spelling follows *Webster's Collegiate Dictionary.*

FORMAT

1. All manuscripts should be formatted in 12-point font on 8 1/2 x 11" paper and should be double-spaced, except for indented quotations.
2. Manuscripts should be as concise as the subject and research method permit, generally not to exceed 7,000 words.
3. Margins should be at least one inch from top, bottom, and sides.
4. To promote anonymous review, authors should not identify themselves directly or indirectly in their papers or in experimental test instruments included with the submission. Single authors should not use the editorial "we."
5. A cover page should show the title of the paper, all authors' names, titles and affiliations, email addresses, and any acknowledgments.
6. The American Accounting Association encourages use of gender-neutral language in its publications.

Pagination: All pages, including tables, appendices and references, should be serially numbered. Major sections should be numbered in Roman numerals. Subsections should not be numbered.

Numbers: Spell out numbers from one to ten, except when used in tables and lists, and when used with mathematical, statistical, scientific, or technical units and quantities, such as distances, weights and measures. For example: *three days; 3 kilometers; 30 years.* All other numbers are expressed numerically.

Percentages and Decimal Fractions: In nontechnical copy use the word percent in the text.

Hyphens: Use a hyphen to join unit modifiers or to clarify usage. For example: *a cross-sectional equation; re-form.* See *Webster's* for correct usage.

Keywords: The abstract must be followed by at least three keywords to assist in indexing the paper and identifying qualified reviewers.

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An Abstract of about 100 words (150 maximum) should be presented on a separate page immediately preceeding the text. The Abstract should concisely inform the reader of the manuscript's topic, its methods, and its findings. Keywords and the Data Availability statements should follow the Abstract. The text of the paper should start with a section labeled "I. Introduction," which provides more details about the paper's purpose, motivation, methodology, and findings. Both the Abstract and the Introduction should be relatively nontechnical, yet clear enough for an informed reader to understand the manuscript's contribution. The manuscript's title, but neither the author's name nor other identification designations, should appear on the Abstract page.

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The author should note the following general requirements:

1. Each table and figure (graphic) should appear on a separate page and should be placed at the end of the text. Each should bear an Arabic number and a complete title indicating the exact contents of the table or figure. Tables and figures should define each variable. The titles and definitions should be sufficiently detailed to enable the reader to interpret the tables and figures without reference to the text.
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Citations: Within-text citations are made using an author-year format. Cited works must correspond to the list of works listed in the "References" section. Authors should make an effort to include the relevant page numbers in the within-text citations.

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2. Unless confusion would result, do not use "p." or "pp." before page numbers. For example, (Dechow and Dichev 2002, 41–42).
3. For cited works that include more than one work by an author (or same co-authors) that are published in the same year, the suffix a, b, etc., is to follow the date in the within-text citations and in the "References" section. For example: (Baiman and Rajan 2002a, 2002b).
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The deadline for free position ads to be included in this section is two months prior to the date of publication. *The Accounting Review* is published in January, March, May, July, September, and November. Position ads in the "Placement Ads" are free with the purchase of a job posting in the AAA Career Center. For more information on how to purchase a job posting in the Career Center and receive your free position ad in *The Accounting Review*, go to the AAA website at http://aaahq.org and click on "Career Center," or call our office at 941-921-7747.

101 UNIVERSITY OF MISSOURI–COLUMBIA, School of Accountancy is seeking applications for a tenure-track faculty position at the Assistant Professor level for Fall 2010. Candidates for the position must have an interest in Financial Accounting, Auditing, or Tax, and be committed to high-quality scholarly research. Applications are encouraged from experienced Assistant Professors as well as new Ph.D.s or those nearing completion of their Ph.D. Salary and research support are competitive. Please send letter of interest, *vita*, three reference letters, evidence of teaching competence, and a sample of working papers to: Dr. Vairam Arunachalam, Director, School of Accountancy, University of Missouri, 303-B Cornell Hall, Columbia MO 65211. Screening will begin immediately and continue until the position is filled. The University of Missouri is an Equal Opportunity/Affirmative Action Employer. Applications from minority and women candidates are strongly encouraged for this position. To request ADA accommodations, please contact the University's ADA Coordinator at (573) 884-7278 (V/TTY).

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103 THE GEORGE WASHINGTON UNIVERSITY, School of Business, Department of Accountancy of invites applications to fill one tenured/tenure-track, open rank position, to begin Fall 2010. Basic Qualifications: Applicants for Associate or Full Professor must have a Ph.D., a distinguished record of research including publications in top academic journals, an excellent record of teaching, and continuing interest in conducting significant scholarly research and guiding junior faculty and doctoral students. Tenure, rank, and salary are commensurate with the qualifications of the successful candidate. Applicants for Assistant Professor must have completed all doctoral degree requirements by August 1, 2010. ABDs will be considered at the rank of Instructor and, if appointed, must complete the doctoral degree by the end of the first academic year of appointment. Applicants must have a strong commitment and capability for scholarly research and teaching excellence. Salary and benefits are competitive. Application procedure: To be considered, applicants should send an application letter, resume, and current working papers to: Professor Keith E. Smith, Chair, Department of Accountancy, School of Business, The George Washington University, Washington, DC 20052; Email: accyjob@gwu.edu. Only complete applications will be considered. Review of applications will begin on November 2, 2009 and will continue until the positions are filled. The George Washington University is an Equal Opportunity/Affirmative Action Employer.

104 CHATHAM UNIVERSITY invites applicants for a faculty position in Accounting starting in Spring 2010. This is a nine-month, renewable term position. Rank is open. Candidates must be aware of latest advancements in the field, have college teaching experience, and possess strong leadership and communication skills. Highly qualified candidates will have experience as team members in successful programs either in an academic or corporate environment. Chatham faculty is expected to provide excellent teaching and service and to maintain a research agenda. Candidate must have a CPA, with a Ph.D. being preferred. Complete application will consist of a letter of interest; *curriculum vitae*; names, addresses, phone numbers, emails of three references; a statement of teaching, research, and service philosophies. Official transcripts will be required for all finalists. Applicants should send the required documents to: Chatham University, Attn: H.R. Dept., Pos. #1117, Woodland Road, Pittsburgh, PA 15232; Email: chathr@chatham.edu Visit: www.chatham.edu.

105 KENT STATE UNIVERSITY, Department of Accounting, invites applications for two tenure-track positions at the Assistant, Associate, or Full Professor level beginning Fall 2010. We are primarily interested in candidates with teaching and research interests in Managerial Accounting, Accounting Information Systems, and Financial Accounting, but will consider all qualified candidates. Candidates should have a Ph.D. with a major in accounting or an appropriate related field. Professional certification is highly desirable. Research will be expected in high-quality, academic refereed journals; service on doctoral students' dissertations will be part of the faculty member's responsibilities. Candidates for an Associate or Full position should have an established scholarly record. Review of applications will begin immediately and continue until the positions are filled. Applicants should send a cover letter and *curriculum vitae* to: Mark Altieri, Chair Faculty Recruiting, Department of Accounting, Kent State University, PO Box 5190, Kent, OH 44242; Email: maltieri@kent.edu. AA/EOE.

106 EASTERN WASHINGTON UNIVERSITY, College of Business and Public Administration, AACSB-accredited, is currently seeking to fill two tenure-track positions at the Assistant and/or Associate Professor level. Appointments require a Ph.D. or D.B.A. in accounting or closely related field, outstanding record of scholarship, strong research potential and/or evidence of research. Individuals will be teaching both undergraduate and graduate courses in all accounting disciplines other than tax. We offer a highly supportive research environment and easy interaction with faculty and other disciplines. Salary and benefits are competitive. Eastern Washington University is a growing and dynamic public comprehensive university co-located in Cheney and Spokane with an enrollment of approximately 10,000 students. Submit letter of application, *curriculum vitae* (including five references), evidence of scholarly activities (including samples), and evidence of effective teaching (including teaching evaluations and a teaching philosophy statement) to: Faculty Chair, Faculty Selection Committee, c/o Ms. Debra Jennings, College of Business and Public Administration, Eastern Washington University, 668 North Riverpoint Blvd., Suite A, Spokane, WA 99202-1668. The successful candidate will be required to pass a background check and show proof of eligibility to work in the U.S. pursuant to U.S. immigration laws. We are an Equal Opportunity/Affirmative Action Employer, and applications from members of historically underrepresented groups are encouraged.

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108 HONG KONG BAPTIST UNIVERSITY, Department of Accountancy and Law, invites applications for positions of Chair/Full Professor/Associate Professor/Assistant Professor in Accounting (PR079/09-10). Applicants must hold, or expect to hold, a Ph.D. degree. Applicants should be able to demonstrate commitment to excellence in research and teaching. Evidence of ability, or potential, to publish in top-tier journals is required. Relevant teaching experience in Financial Accounting, Managerial Accounting, or Auditing is preferred. Supervision of Ph.D. and M.Phil. students and program administration is expected for Chair/Full Professorship. Appointments will normally be made on contract basis for up to three years initially commencing September 2010. Appointment is renewable and substantiable subject to review and mutual agreement. Applicants should provide an updated *cv* and/or complete the application form downloadable from: http://www.hkbu.edu.hk/~pers. All applicants should send in their most recent teaching evaluation results and request two referees to send in confidential letters of reference to the Personnel Office direct. Samples of publications, preferably three best ones of most recent publications should also be sent. Applications should be sent to: Personnel Office, Hong Kong Baptist University, Kowloon Tong, Hong Kong. Please quote PR number on all correspondence. The closing date for application is December 10, 2009 or until the positions are filled.

109 UNIVERSITY OF NORTHERN BRITISH COLUMBIA, BC School of Business, College of Science and Management invites applications for an Assistant/Associate Professor tenure-track position in Accounting with an anticipated start date of July 1, 2010. The successful candidate will possess a Ph.D. in any area of Accounting. A professional designation is an asset. Applicants will be evaluated on their potential for and/or record of excellence in research, teaching, and service. Candidates at the Associate rank should have demonstrated a strong record of high-quality performance in each of these areas. Although the position is open to all areas of Accounting, the successful candidates must be able to deliver teaching in the area of Management Accounting. For a listing of qualifications and job description please visit: http://www.unbc.ca/hr/postings/noninstructional.html. Applications received on or before November 30, 2009 will receive full consideration.

110 UNIVERSITY OF WISCONSIN–WHITEWATER, Accounting Department, anticipates a full-time, tenure-track position starting Fall 2010. Teaching needs include accounting information systems, auditing, financial accounting, and cost accounting. A Ph.D. or D.B.A., research, advising, and service are required. Salary and rank will depend on experience. Application review will begin on November 15, 2009, and continue until the position is filled. To apply, submit a letter of interest, *curriculum vitae*, and three letters of recommendation in writing to: Dr. Alka Arora, Department of Accounting, HH3301, University of Wisconsin–Whitewater, 800 West Main Street, Whitewater, WI 53190-1790; Email: aroraa@uww.edu. The University of Wisconsin–Whitewater is an Equal Opportunity/Affirmative Action Employer, and actively seeks and encourages applications from women, people of color, persons with disabilities, and all veterans. Names of applicants may be disclosed unless requested otherwise. Names of finalists will be released. A criminal background check will be required of the preferred candidate.

111 BINGHAMTON UNIVERSITY, SUNY, School of Management, AACSB-accredited, invites applications for an Assistant Professor position in Accounting for Fall 2010, pending budget approval. Candidates must have (or expect) a Ph.D. degree by next fall. High-quality research as evidenced by publication in the top-tier accounting journals is expected. Research is supported with a two course/semester teaching assignment. The School of Management also supports research with subscriptions to WRDS and other standard databases. The university offers bachelor's, master's, and Ph.D. programs in accounting. More details are available at: http://som.binghamton.edu/faculty/index.htm. Located three hours from NYC and one hour from Syracuse and Ithaca, Binghamton is a low-cost-of-living area with 200,000 people. Interested candidates should post a curriculum *vita* and other supporting documents to the appropriate position listing at: http://binghamton.interviewexchange.com/: Binghamton University is an Equal Opportunity/Affirmative Action Employer. Application materials must be submitted electronically.

112 NANYANG TECHNOLOGICAL UNIVERSITY, Nanyang Business School, Division of Accounting invites applications for tenure-track Assistant Professor and Associate Professor positions, as well as non-tenure-track Lecturer and Senior Lecturer positions in all areas of Accounting. Applicants for Assistant Professor and Associate Professor positions should possess a Ph.D. in accounting, demonstrate or show promise of scholarly research publications in top accounting journals, and have a good teaching record. Applications for Lecturer and Senior Lecturer positions should have a post-graduate degree in accountancy, membership with a reputable accountancy professional body, and a good teaching record. Interested individuals are encouraged to email their cover letter, *curriculum vitae*, working papers, teaching record, and three letters of recommendation, to: Faculty Search Committee, Division of Accounting, Nanyang Business School, Nanyang Technological University, S3-01-07/08, Nanyang Avenue, Singapore 639798: Email: NBS_Search@ntu.edu.sg.

113 NATIONAL UNIVERSITY OF SINGAPORE, Department of Accounting, NUS Business School invites applications for tenure-track positions at all ranks (Assistant/Associate/Full). All accounting-related disciplines will be considered. Candidates for the junior rank should possess a Ph.D. or be close to completion. NUS is a research-intensive university, ranked 30th among the world's top 100 universities by the *Times Higher Education* in 2009. NUS Business School research is ranked 47th worldwide according to the UT Dallas Top 100 Worldwide Business School Rankings. Our school has a wide collection of databases including WRDS, Compustat, CRSP, Datastream, and I/B/E/S, and provides excellent support for research and for attending conferences. Our teaching load is comparable to those offered in top U.S. schools, and further reduced for Assistant Professors. Our accounting faculty have published in leading journals such as *CAR, JAR, JAE, JFE, RAST*, and *TAR*. To promote active interactions with researchers outside Singapore, our department regularly invites and receives visits by top accounting researchers, including Ashiq Ali, Mark DeFond, Dan Dhaliwal, Peter Easton, Wayne Landsman, and Douglas Skinner. The visitors stay at NUS normally for one to two weeks, and in some cases for several months so that our faculty have ample opportunities to interact with those visitors. Our pay package compares favorably with those offered by top U.S. schools. Apart from pecuniary benefits, expatriate faculty members receive generous housing subsidies as well as education subsidies for children attending world-class international schools in Singapore. Singapore is a low-tax country with an average effective tax rate of around 15 percent. Interested parties are urged to send *cv*, research papers, and three letters of recommendation to: Ms Lam Yin Wah, Coordinator, Recruiting Committee, Department of Accounting, NUS Business School, National University of Singapore, One Business Link, Singapore 117592; Phone: (65) 6516-7798; Fax: (65) 6779-2083; Email: accbox1@nus.edu.sg. Visit: http://www.bschool.nus.edu.

114 ADELPHI UNIVERSITY ACCOUNTING, School of Business, AACSB-accredited, has an exciting agenda of change and growth. We are seeking outstanding individuals for two positions at the Assistant/Associate Professor, tenure-track, to teach introductory and advanced undergraduate and graduate courses in Accounting. Each position requires an earned doctorate in the teaching discipline; a record of scholarly publications; a demonstrated record of, and commitment to, outstanding teaching; and professional experience. The successful candidates will have teaching/research interests in financial accounting (especially, interest in teaching Intermediate Accounting). Ability to teach accounting information systems is a plus. CPA or CMA designation strongly preferred. Apply online at: http://www.adelphi.edu/positions/faculty.

115 MIAMI UNIVERSITY, Department of Accountancy invites applicants for a non-tenure-track position at the Clinical Faculty level beginning Fall 2010. This position primarily is to teach and coordinate the introductory financial accounting course as well as provide service to the department. At the minimum, applicants must have an accounting related master's degree and university-level teaching experience. A doctorate, professional certification, and relevant professional work experience are desirable. Salary and benefits are competitive. Both the undergraduate and graduate accounting programs are AACSB-accredited. Faculty must be either academically or professionally qualified. Please submit resume and letters from three references to: Dr. Marc A. Rubin, Department of Accountancy, Farmer School of Business, Miami University, Oxford, OH 45056; or Email: rubinma@muohio.edu. Screening of applications begins October 15, 2009 and will continue until the position is filled. Information about the faculty and curriculum is available at: http://www.sba.muohio.edu. Miami University is an EOE/AA Employer with smoke-free campuses. Campus crime and safety Report at: http://www.muohio.edu/righttoknow. Hard copy upon request.

116 WASHINGTON UNIVERSITY IN ST. LOUIS, Olin Business School is seeking candidates to fill a faculty position at the tenured or advanced tenure-track level beginning in Fall 2010 to teach courses in Accounting and/or related areas, conduct research, publish in peer-reviewed journals, and participate in faculty service activities. Candidates are expected to have a substantial record of scholarship and an international reputation. Hiring is contingent on available funding. Applications are being accepted through the Washington University in St. Louis Faculty Recruitment link. Please visit our website, http://www.wustlcareers.com, and click on the Search Posting/Apply tab on the left panel; use the search criteria to review complete descriptions of positions and guidelines for applying. Application review will continue until the position is filled. Olin Business School fosters and appreciates ethnic and cultural diversity among its faculty, students and staff. Applications from men, women, ethnic minorities, veterans, dual-career couples, and individuals with disabilities are encouraged to apply.

117 UNIVERSITY OF SOUTH FLORIDA, School of Accountancy invites applicants for two tenure-earning Assistant/Associate Professors beginning Fall 2010. These positions will involve teaching in undergraduate and master's programs and involvement with Ph.D. program. Other duties include conducting research leading to publications in refereed journals and service to the academic and professional communities. A Ph.D. in Accounting or near completion from an AACSB-accredited institution is required. All specialties within accounting will be considered. For more complete description of position including minimum and preferred qualifications contact: Dr. Stephanie M. Bryant, School of Accountancy, College of Business, University of South Florida, 4202 E. Fowler Avenue (BSN3403), Tampa, FL 33620-5500; Phone: (813) 974-6516; Fax (813) 974-6528; Email: sbryant@coba.usf.edu. The search will continue until the positions are filled. Positions are subject to final university funding. USF is an Equal Opportunity/Affirmative Action Institution. For disability accommodations, call (813) 974-6389 a minimum of five working days in advance. Under Florida law, applications and meetings are public. Applications must be completed online at: http://www.usf.edu/jobs

118 JOHNS HOPKINS UNIVERSITY, Carey Business School invites nominations and applications from faculty who are motivated by the unique opportunity to build a world-class business school. Non-tenure-track full-time appointments are currently available in Accounting, Finance, and Management. Each candidate must possess a doctoral degree from an accredited university, a strong record of academic research, and excellent teaching skills. Experience in healthcare, life-sciences, or biological sciences industries is preferred. For full consideration, candidates must apply electronically by submitting a cover letter outlining area of interest, *curriculum vitae*, and the names, addresses, and phone numbers of three references to: Dr. Phillip Phan, c/o John J. Kunz, Director of Human Resources. Documents should be in Word or PDF format and emailed: to careyschool-hr@jhu.edu. Review of candidates has commenced. Positions will remain open until filled. Johns Hopkins is an Equal Opportunity/Affirmative Action Employer. Minorities and women are strongly encouraged to apply.

119 NORTHERN MICHIGAN UNIVERSITY, College of Business seeks candidates for a Instructor/ Assistant Professor in Accounting (Taxation) three-year term faculty position beginning August 2010. Teaching includes federal taxation, other accounting courses, possibly financial planning. Responsibilities include advising, program development, outcomes assessment, scholarship, and service. AACSB-accredited; scholarly publications are expected. Minimum requirement is appropriate Master's Degree. CPA highly desirable. Assistant Professor level requires Ph.D. in an appropriate discipline from AACSB university or J.D. from ABA-accredited institution plus taxation credentials. We offer all standard business discipline Bachelor's degree programs plus four Associate degree programs on a mid-size campus. Marquette, Michigan, an award-winning community on the beautiful southern shore of Lake Superior, is the cultural, economic, and medical center of Michigan's Upper Peninsula. Recreational activities are ideal during all seasons. Review begins November 1. Position posted until January 12, 2010. Apply online with application letter, *cv*, transcripts, and contact information for three references. Apply at: https://employMe.nmu.edu. NMU is an EOE.

120 UNIVERSITY OF CINCINNATI, Department of Accounting invites applications for Department Head beginning Fall 2010. The successful applicant must qualify for tenure in the College of Business. The Department currently has eight full-time, tenure-track faculty and two full-time, non-tenure-track faculty in Accounting and two full-time, non-tenure-track faculty in Business Law. Minimum qualifications: Ph.D. in Accounting. Candidates must show excellence in research and teaching and a commitment to professional service. The College of Business is AACSB-accredited at the B.B.A. and M.B.A. degree levels and offers a Ph.D. program. In addition to a B.B.A. in Accounting, an M.S. degree in Accounting is offered. Submit your letter of application and *curriculum vita* at: http://www.jobsatuc.com, position number 295427. The University has a strong commitment to the principle of diversity and seeks a broad spectrum of candidates.

121 UNIVERSITY OF HOUSTON, Department of Accountancy and Taxation at the C.T. Bauer College of Business invites applications for three tenured or tenure-track appointments at all ranks for Fall 2010. Applicants for senior positions should have a national reputation for scholarship, and a record of excellence in teaching at the undergraduate and graduate levels. Applicants for junior positions must have a doctoral degree prior to the appointment date and exhibit strong potential for and commitment to excellence in research and teaching. We are seeking applications in all areas of Accounting. The C.T. Bauer College of Business and the department are AACSB-accredited. Salaries are very competitive. Please send a copy of your resume, research papers, and relevant teaching materials to: Dr. Gerald Lobo, Department Chair, Department of Accountancy and Taxation, University of Houston, C.T. Bauer College of Business, 334 Melcher Hall, Houston, TX 77204-6021; or Email: acctrecruiting@bauer.uh.edu. The University of Houston is an Equal Opportunity/Affirmative Action Employer. Women, minorities, veterans, and individuals with disabilities are encouraged to apply.

122 CORNERSTONE RESEARCH, an economic consulting firm specializing in analyzing economic, financial, accounting, and marketing issues, is interested in hiring full-time Associates for its offices in Boston, MA, Los Angeles, CA, Menlo Park, CA, New York, NY, San Francisco, CA, and Washington, D.C. Cornerstone Research provides an interesting and rewarding work environment. Associates work directly with esteemed faculty experts in a distinctive "partnership" that combines the strengths of the academic and business worlds. We seek Ph.D. candidates with the ability to apply academic research to real-world issues, present concise explanations of complex analyses, and independently manage projects and junior staff. The ideal candidate will possess a strong empirical background in Financial or Cost Accounting, or Auditing, as well as demonstrate excellent interpersonal skills. We also seek experienced candidates for more senior levels. Compensation is commensurate with experience. Interested candidates should submit a cover letter (that includes a statement describing your interest in economic and financial consulting and your location preferences), resume, and a sample research paper to: http://Associatecareer.cornerstone.com. Recruiting Administrator-AR website: http://www.cornerstone.com.

123 UNIVERSITY OF CALIFORNIA, IRVINE, The Paul Merage School of Business is seeking candidates for a tenure-track Assistant Professor of Accounting to begin July 1, 2010. All candidates will be considered with preference to those who can teach managerial accounting. Required qualifications include a demonstrated interest and capacity to do research publishable in top-tier accounting journals, high level of teaching competence, and an earned or near completed doctorate. We offer a competitive compensation package. For details about UCIrvine and the School, application procedures, and deadline, visit http://www.merage.uci.edu/. Under federal law, the University of California may employ only individuals who are legally authorized to work in the United States as established by providing documents specified in the Immigration Reform and Control Act of 1986. The University of California, Irvine is an Equal Opportunity Employer committed to excellence through diversity and strongly encourages applications from all qualified applicants, including women and minorities.

124 VALDOSTA STATE UNIVERSITY (fully accredited at the undergraduate and graduate level) seeks candidates for an Assistant Professor of Accounting position. All teaching areas considered. VSU offers a B.B.A. in Accounting and an M.Acc. at the graduate level. Contact: Dr Seat; Phone: 229-245-3813; Email: donseat@valdosta.edu for more information.

125 SIENA COLLEGE, Accounting Department invites applications for a tenure-track position beginning September 2010. Applications will be accepted and considered until the position is filled. Rank and salary are dependent upon experience and credentials. A Ph.D./D.B.A. with concentration in accounting is required; relevant business experience is desirable. An experienced tax professional with an L.L.M. is acceptable for someone desiring a position teaching tax. Research and professional development activities appropriate to academically qualified status for AACSB accreditation and School of Business mission are necessary. Siena College is an independent liberal arts college in the Catholic and Franciscan tradition, located in New York's Capital District, with close proximity to major higher education and cultural institutions. The College enrolls 3,000 full-time students; 1,200 in the School of Business. Please forward applications to: Accounting Search Committee, c/o School of Business, Siena College, 515 Loudon Road, Loudonville, NY 12211-1462. Siena College is an Equal Opportunity Employer and encourages applications from all qualified candidates.

126 UNIVERSITY OF ARKANSAS, Department of Accounting, Sam M. Walton College of Business seeks an Assistant/Associate Professor (tenure-track position) beginning August 2010 and will consider all teaching interests. Candidates for Assistant/Associate Professor should have a Ph.D. in accounting, outstanding research potential, and a strong commitment to excellence in graduate and undergraduate teaching. This position is contingent on funding. The University of Arkansas is a nationally competitive, student-centered, research university serving Arkansas and the world. The city of Fayetteville is located in the northwest corner of the state. It is a city of approximately 70,000 with a regional population of more than 400,000. The region boasts one of the highest growth rates and is consistently listed among the most desirable places to live in the U.S. A number of *Fortune* 500 companies are headquartered in northwest Arkansas, including Wal-Mart, Tyson, and J.B. Hunt. Interested candidates should send: (1) a current *vitae*; (2) evidence of teaching effectiveness; (3) copies of research working papers; and (4) letters of reference or the name, address, telephone number, and email address of at least three references to: Linda Myers, Department of Accounting, WCOB 401, Sam M. Walton College of Business, One University of Arkansas, Fayetteville, AK 72701. For more information contact Dr. Myers at: lmyers@walton.uark.edu. The University of Arkansas is an Equal Opportunity/Affirmative Action Employer. All applicants are subject to public disclosure under the Arkansas Freedom of Information Act and persons hired must have proof of legal authority to work in the United States.

127 CALIFORNIA STATE UNIVERSITY, SAN BERNARDINO, Department of Accounting and Finance seeks candidates for an Assistant Professor, but rank is open depending on qualifications. Ph.D. in accounting or academically qualified in accounting according to AACSB standards as defined by CSUSB. Industry experience and CPA a plus. All areas of accounting are considered, but preference will be given to candidates with teaching and research experience in financial accounting, auditing, or governmental accounting. Candidates at the senior academic ranks are expected to teach in the new Master of Science in Accountancy degree program of the department. Preferred candidates will be expected to meet the traditional requirements of excellence in teaching, active scholarly and professional work, and service to the University and community. Deadline and Application Process: Review of applications will begin on November 30, 2009 and continue until the position is filled. Submit *vita* with letter of application that includes statement on teaching philosophy and strategies, research/professional accomplishments, and goals. Also submit the names, phone numbers, and email addresses of three references, along with an official copy of most recent transcripts (unofficial copies acceptable for initial screening). Please include copies of representative publications and evidence of teaching performance if available to: Dr. Ghulam Sarwar, Chair Department of Accounting and Finance, College of Business and Public Administration, 5500 University Parkway, San Bernardino, CA 92407-2397; Email: gsarwar@csusb.edu.

128 THE UNIVERSITY OF TEXAS AT DALLAS, Accounting and Information Management School of Management invites applications for an open-rank tenure-system position in Accounting (pmy090914); rank will be based on the candidate's qualifications. The finalist will hold a Ph.D. and have, or demonstrate the potential to achieve, a strong record of research and scholarly publication. The School of Management is a research-centered school, offers degrees at the Ph.D., Master's, and Undergraduate levels, and employs a nationally ranked and recognized faculty. Appointments will be effective September 1, 2010. Salary and research support will be competitive. Review of applications will begin immediately and will continue until May 1, 2010, or the position is filled. Applicants should submit resume or *curriculum vitae* (educational history, teaching, research, pertinent work experience, and list of publications and presentations) and contact information (or letters of support) from five professional references via the online application available at: http://go.utdallas.edu/pmy090914. The University of Texas at Dallas is an Equal Opportunity/Affirmative Action University and strongly encourages applications from candidates who would enhance the diversity of the University's faculty and administration. All qualified applicants will receive consideration for employment without regard to race, color, sex, national origin, disability, age, citizenship status, Vietnam era, or special disabled veteran's status, or sexual orientation. Indication of gender and ethnicity for affirmative action statistical reporting purposes is requested but not required for consideration.

THE CHINESE UNIVERSITY OF HONG KONG

Applications are invited for:-

School of Accountancy

Professor(s) / Associate Professor(s) / Assistant Professor(s) / Visiting Scholar(s)

(Ref. 0910/063(020)/2)

Applicants should have a PhD degree (or close to completion) and strong commitment to scholarly research and teaching. The appointee(s) will teach in the areas of financial accounting, management accounting, auditing, taxation, and/or accounting information system. Appointment(s) will normally be made on contract basis for one to three years initially commencing August 2010, which, subject to mutual agreement, may lead to longer-term appointment or substantiation later. Short-term appointment(s) as Visiting Scholar(s) may also be arranged. Applications will be accepted until the post(s) are filled.

Salary and Fringe Benefits
Salary will be highly competitive, commensurate with qualifications and experience. The University offers a comprehensive fringe benefit package including medical care, plus a contract-end gratuity for appointment(s) of two years or longer; and housing benefits for eligible appointee(s). Further information about the University and the general terms of service for appointments is available at *http://www.cuhk.edu.hk/personnel*. The terms mentioned herein are for reference only and are subject to revision by the University.

Application Procedure
Please send full resume, a publication list and abstracts of selected published papers, and pertinent application materials to Professor Danqing Young, Director, School of Accountancy, The Chinese University of Hong Kong, Shatin, N.T., Hong Kong or e-mail to *dyoung@baf.msmail.cuhk.edu.hk*. The Personal Information Collection Statement will be provided upon request. Please quote the reference number and mark ‘Application - Confidentialon’ cover.

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